QUARTERLY JOURNAL
OF THE
ROYAL METEOROLOGICAL SOCIETY

Editor: T. W. WORMELL

Vol. 80 JULY 1954 No. 345

CONTENTS

PAGE

Reviews of Modern Meteorology—13. The physics of rainclouds – I. C. Browne, H. P. Palmer and T. W. Wormell (Plate IV) 291

O. G. SUTTON—Presidential Address—The development of meteorology as an exact science 328

G. K. BATCHELOR—Symons Memorial Lecture—Heat convection and buoyancy effects in fluids (Plate V) 339

R. A. McCORMICK—The partition and intensity of eddy energy at the 91-metre level during unstable conditions as observed at Brookhaven National Laboratory 359

F. J. SCRASE—Turbulence in the upper air, as shown by radar-wind and radiosonde measurements 369

R. F. JONES—Five flights through a thunderstorm belt 377

B. J. MASON and R. RAMANADHAM—Modification of the size distribution of falling raindrops by coalescence 388

I. J. W. POTHECARY—Short-period variations in surface pressure and wind .. 395

E. J. SUMNER—A study of blocking in the Atlantic-European sector of the northern hemisphere 402

R. S. SCORER—Theory of airflow over mountains : III – Airstream characteristics 417

Shorter Contributions—

D. R. DAVIES—A note on the two-dimensional equation of diffusion in the atmosphere—Discussion 429

T. H. ELLISON—A note on the use of the stereographic map projection in dynamical meteorology 435

J. R. D. FRANCIS—Wind stress over a water surface 438

G. REYNOLDS—Snowfall probabilities on Merseyside 444

E. TROLLUND A.—A simple mechanism to improve the sensitivity of a Dines anemograph 447

W. W. MAPLESON—The drag of spherical rubber balloons 449

Correspondence—

G. N. PLASS; A. R. CURTIS and R. M. GOODY—Spectral line shape and its effect on atmospheric transmissions 452

S. C. MOSSOP; E. K. BIGG—The freezing of cloud droplets 455

J. L. MONTEITH; W. E. SAUNDERS—Night cooling under clear skies 457

Discussions 460

W. J. GIBBS—A comparison of hemispheric circulations with particular reference to the Western Pacific (*Q.J.*, **79**, p. 121); J. SMAGORINSKY—The dynamical influence of large-scale heat sources and sinks on the quasi-stationary mean motions of the atmosphere (*Q.J.*, **79**, p. 342); C. V. SMITH and A. G. FORSDYKE—Some downstream effects associated with large-amplitude troughs in upper flow patterns (*Q.J.*, **79**, p. 414); R. M. GOODY and C. D. WALSHAW—The origin of atmospheric nitrous oxide (*Q.J.*, **79**, p. 496); R. S. SCORER—The nature of convection as revealed by soaring birds and dragonflies (*Q.J.*, **80**, p. 68).

Report of the Council for 1953 468

Proceedings at meetings of the Society 478

Reviews 482

Koninklijk Nederlands Meteorologisch Instituut 1854-1954 (P.A.S.); *Acta Geophysica Polonica*, Vol. I (G.A.B.); *Memorandum by the National Smoke Abatement Society to the Committee on Air Pollution* (P.A.S.); *Annual Report of the Director of the Meteorological Office, April 1952-March 1953* (C.D.O.); *Meteorological Instruments*, W. E. K. Middleton and A. F. Spilhaus (R.J.M.); *Die Grundschicht der Troposphäre*, K. Schneider-Carius (A.G.F.); *Methods of mathematical physics*, Vol. I, R. Courant and D. Hilbert (E.K.); *A textbook of radar*, Ed., E. G. Bowen (J.E.N.H.); *Die Wolken*, R. Süring (F.H.L.); *Atomic energy; a survey*, Ed., J. Rotblat (F.H.L.); *English weathervanes; their stories and legends from medieval to modern times*, A. Needham (L.C.W.B.); *Linke's Meteorologisches Taschenbuch*, Bd. II (G.D.R.); *Grundriss einer Meteorobiologie des Menschen*, B. de Rudder (N.P., R.E.H.M.); *Maps and diagrams*, F. J. Monkhouse and H. R. Wilkinson (N.P.); *Westward Ho with the 'Albatross,'* H. Pettersson (C.D.O.)

Obituary Notices 489

PUBLISHED AND SOLD BY THE
ROYAL METEOROLOGICAL SOCIETY
49 CROMWELL ROAD, LONDON, S.W.7
PRICE TWENTY SHILLINGS

ROYAL METEOROLOGICAL SOCIETY

Founded on 3 April 1850, for the advancement of meteorological science. The Society was incorporated by Royal Charter in 1866.

Membership

Membership is open to all persons genuinely interested in meteorology. The annual subscription is as follows :

Fellows	£3-3-0	(Entrance fee, 1 guinea)
Foreign Members ..	£3-3-0	„ „
Associates	£2-0-0	„ „
Students	£1-10-0	

All Members receive free the regular publications of the Society, including the monthly publication *Weather* (annual subscription to non-members 24s.).

Quarterly Journal

Editing committee

T. W. Wormell — *Editor and Chairman*

J. K. Bannon G. Manley G. D. Robinson R. C. Sutcliffe
G. A. Bull H. L. Penman P. A. Sheppard

The primary purpose of the *Quarterly Journal* is to publish results of original research and reports of the Society's scientific meetings. Papers intended for the *Journal* **should be submitted in accordance with standards laid down by the Council in the Society's memorandum** ' Preparation of Papers for the *Quarterly Journal* ' which is available on request. Failure to conform with these standards involves the Society's officers and staff in much needless work and the Society in unnecessary expense. **Manuscripts will therefore not be considered for publication unless they conform reasonably closely with the procedure laid down.** The attention of authors is drawn particularly to the requirements in regard to mathematics and diagrams and the Assistant Secretary will be pleased to advise authors on their special problems of illustration before papers are formally submitted. Diagrams should, if possible, be submitted flat, not rolled, and a duplicate rough set for refereeing purposes should be included. Where rulings on a diagram are desirable the closest acceptable spacing is approximately 1 cm and this may be done on **blue-lined** graph paper by thickening the centimetre rulings. Manuscripts may be submitted in duplicate to expedite refereeing, but authors must retain complete copies in their possession.

Price to non-members is £3-1-0 (post free) per annum and £1-0-0 for single issues.

Publications

Copies of the publication *Centenary Proceedings of the Royal Meteorological Society*, 1950 are still available, price £1. 1s.

Reprints of papers recently published in the *Quarterly Journal* are generally available, the prices being as follows (postage extra) : 8 pp., 1s. 2d. ; 16 pp., 1s. 10d. ; 20 pp., 2s. 2d. Reprints in the series ' Reviews of Modern Meteorology ' published to date are available : 1. Meteorological measurements (3s. 2d.) ; 2. Radiation in the troposphere and lower stratosphere (3s. 10d.) ; 3. Dynamics of flow patterns in extra-tropical regions (2s. 6d.) ; 4. The evolution of 20th-century forecasting in the British Isles (2s. 6d.) ; 5. Tropical meteorology (3s. 10d.); 6. Principles of synoptic weather forecasting (3s. 2d.); 7. Turbulence and transfer processes in the atmosphere (4s. 2d.); 8. Atmospheric electricity, some recent trends and problems (3s. 6d.) ; 9. Climatic variation (2s. 7d.) ; 10. Convection in the atmosphere (2s. 10d.); 11. The physical state of the upper atmosphere (1s. 10d.); 12. The problem of tropical hurricanes (3s. 6d.); 13. Cloud physics, by I. C. Browne, H. P. Palmer and T. W. Wormell (4s.); Forthcoming review in this series : 14. General circulation, by G. D. Robinson, P. A. Sheppard and R. C. Sutcliffe.

The Society's offices

Assistant Secretary — M. E. Shoenberg, B.A.
49 Cromwell Road, London, S.W.7
(*Telephone :* KENsington 0730)

The offices and library of the Society are open daily from 10 a.m. to 5 p.m., Saturdays 9.30 a.m. to 12.30 p.m. All enquiries and correspondence concerning the Society and its publications should be addressed to the Assistant Secretary.

This Journal is printed in Horley Series

AS

SUPPLIED

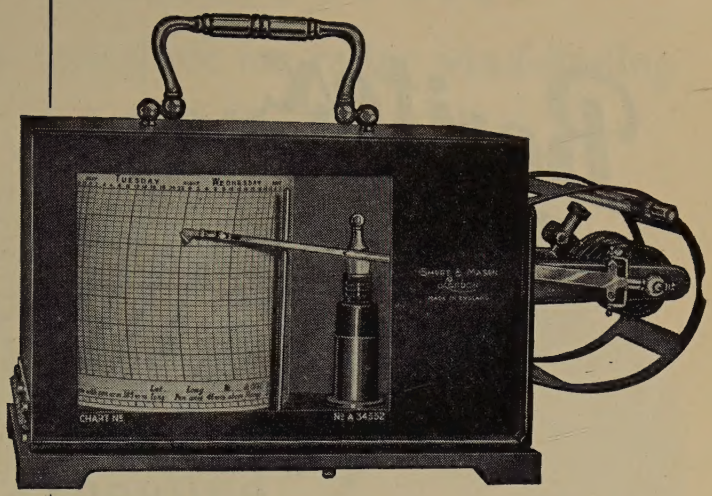
TO

THE

BRITISH

METEOROLOGICAL

OFFICE



This highly sensitive bimetallic coil instrument has a weekly low drum clock. The laminated coil is exposed outside the case to ensure perfect circulation of air.

S. & M. "Gate" suspension on the pen arm eliminates friction between pen and chart. A 6 in. solid glass thermometer is included with the thermograph for setting purposes. The position of the pen on the chart may be altered for different climates or seasons.

RANGES AVAILABLE:—

−10/+70° F. (Winter)
+20/100° F. (Summer)
0/45° C.

Further details and information will be gladly supplied on request. Available for quick delivery.

S & M THERMOGRAPH

SHORT & MASON MEANS ACCURACY FIRST

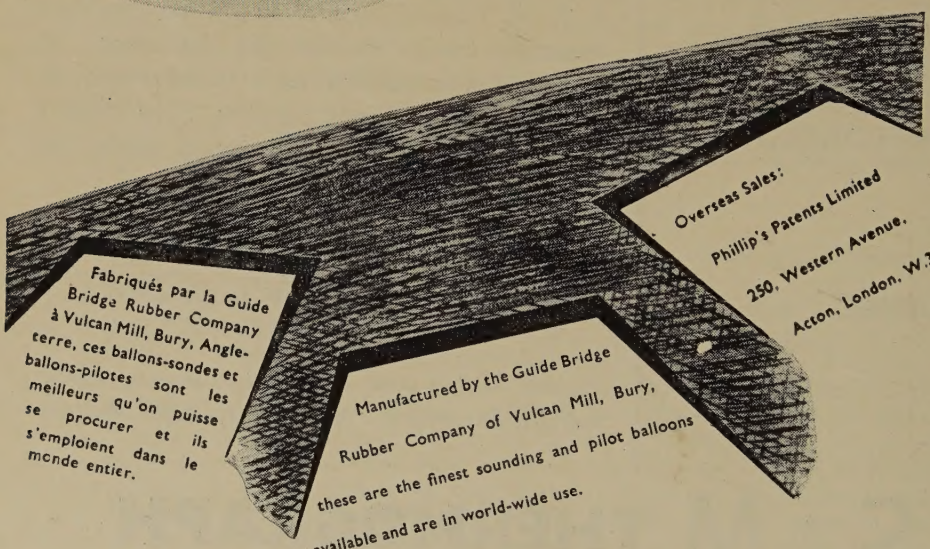
SHORT & MASON

ANEROID WORKS, HALE END RD., WALTHAMSTOW, LONDON, E.17. LARkswood 3371-5

Beritex

REGD SEAMLESS

the finest sounding and pilot balloons


Overseas Sales:
Phillip's Patents Limited
250, Western Avenue,
Acton, London, W.3.
Fabriqués par la Guide Bridge Rubber Company à Vulcan Mill, Bury, Angleterre, ces ballons-sondes et ballons-pilotes sont les meilleurs qu'on puisse se procurer et ils s'emploient dans le monde entier.
Manufactured by the Guide Bridge Rubber Company of Vulcan Mill, Bury, these are the finest sounding and pilot balloons available and are in world-wide use.

HOME SALES

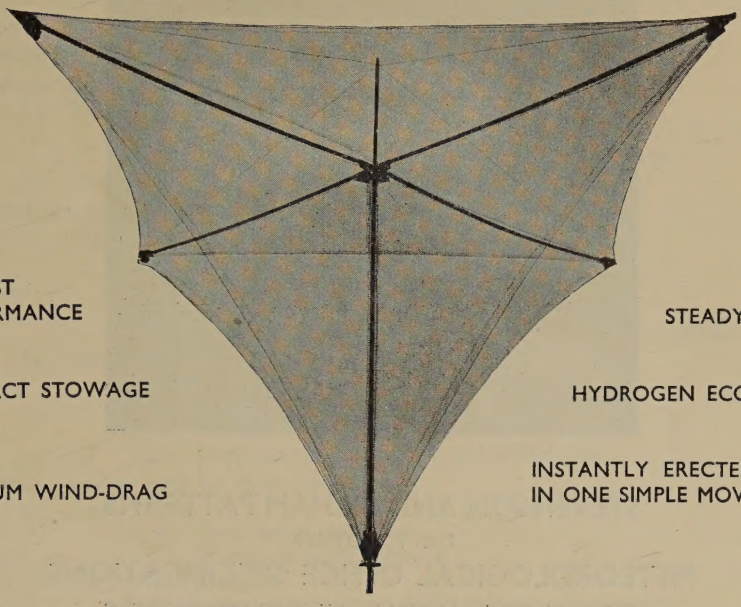
GUIDE BRIDGE RUBBER CO LTD · VULCAN MILL · BUTCHER LANE · BURY · LANCS

UMBRAWIN

The modern technique for radar wind-finding

MARK III

★ FURTHER REDUCTION IN WEIGHT ★
★ GREATER EFFICIENCY ★
★ EXTRA STRENGTH ★



HIGHEST PERFORMANCE

STEADY ECHO

COMPACT STOWAGE

HYDROGEN ECONOMY

MINIMUM WIND-DRAG

INSTANTLY ERECTED IN ONE SIMPLE MOVEMENT

Umbrawin Model	Hypotenuse		Weight (ounces)	Performance (Radar GL III) Range (yards)	Altitude (feet)
	ft.	ins.			
Mark I (steel)	4	6	28	70,000 ± 10,000	45,000-80,000
Mark II (aluminium)	4	6	22½	70,000 ± 10,000	45,000-80,000
Mark III (aluminium)	4	6	17	80,000 ± 10,000	45,000-80,000
Mark X (steel)	3	0	20	60,000 ± 5,000	45,000-80,000
Mark Y (aluminium)	3	0	15	60,000 ± 5,000	45,000-80,000
Mark Z (aluminium)	3	0	12	60,000 ± 5,000	45,000-80,000

Details of Umbrawin and other targets from : Sounding Balloons also supplied

CHEMRING LIMITED

Specialists in Radar Reflectors

Contractors to Air Ministry and other Meteorological authorities throughout the world

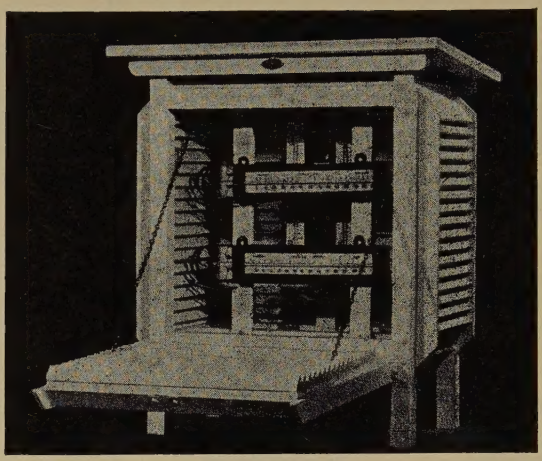
62-64, Temple Chambers, Temple Avenue, London, E.C.4

Telephone : Central 8361 Cables: Sucal, London

CASELLA LONDON

Established 1810

METEOROLOGICAL SCREENS



STEVENSON AND BILHAM PATTERNS
OR TO OTHER
METEOROLOGICAL OFFICE SPECIFICATIONS
COMPLETE WITH THERMOMETERS

These screens, together with full range of thermometers and other meteorological equipment, are available from stock.

Please write for our Catalogue

C. F. CASELLA & CO. LTD.

REGENT HOUSE, FITZROY SQUARE, LONDON, W.1

Telephone : ★ EUSton 3944

Member Scientific Instrument Manufacturers' Association of Great Britain

QUARTERLY JOURNAL
OF THE
ROYAL METEOROLOGICAL SOCIETY

Vol. 80 JULY 1954 No. 345

REVIEWS OF MODERN METEOROLOGY—13

551.501.81 : 551.574.1 : 551.576.11

The physics of rainclouds

By I. C. BROWNE*, H. P. PALMER† and T. W. WORMELL
Jodrell Bank Experimental Station, University of Manchester
Cavendish Laboratory, Cambridge

CONTENTS

1. INTRODUCTION
2. OBSERVATIONAL EVIDENCE
   2.1 The growth of precipitation particles from ice crystals
      2.11 The temperatures of cloud summits
      2.12 Radar observations and cloud-seeding experiments
   2.2 The growth of precipitation particles by coalescence
3. PRECIPITATION ELEMENTS
   3.1 Physical properties of precipitation elements
      3.11 Terminal velocities
      3.12 Collection efficiencies
      3.13 Coalescence of drops
      3.14 The fragmentation of drops and splintering of ice crystals
   3.2 Growth processes
      3.21 Condensation nuclei
      3.22 Ice-forming nuclei
      3.23 The nature of freezing nuclei
      3.24 Condensation, evaporation and sublimation
4. PHYSICAL PROPERTIES OF RAINCLOUDS
   4.1 Layer clouds
      4.11 Cloud depth and extent
      4.12 Updraught and turbulence
      4.13 Humidity
      4.14 Liquid-water content and droplet-size distribution
   4.2 Convective rainclouds
      4.21 Cumulonimbus cells
      4.22 Updraught and turbulence
      4.23 Composition of convective clouds
      4.24 Liquid-water content and drop-size distribution
5. CLOUD BUDGETS
   5.1 Layer clouds
      5.11 The radar echo
      5.12 A model cloud
      5.13 The humidity in ice clouds
      5.14 Discussion
   5.2 Convective rain clouds
      5.21 Macrophysical problems
      5.22 The initiation of rainfall
      5.23 Growth of precipitation elements
6. USE OF RADAR IN CLOUD STUDIES
   6.1 The physics of scattering by meteorological particles
   6.2 Doppler shifts in radar cloud echoes
   6.3 The melting band
7. CONCLUSION
8. ACKNOWLEDGMENTS
   REFERENCES
   APPENDIX

* Formerly at Cavendish Laboratory, Cambridge. † Formerly at Clarendon Laboratory, Oxford.

1. Introduction

The intensive modern study of the physics of precipitation owes much to a classical paper of Bergeron (1933). He emphasized that heavy cloud, without precipitation, is frequently very stable and long-lived, and that the nature of the process leading to the release of precipitation, involving the transfer of a large mass of water from a state of dispersion in very numerous particles to one of concentration in a much smaller number of much larger particles, remained to be explained. After discussing various possibilities, he concluded that the only process of widespread significance, which would lead to the formation of ' real ' rain and snow, involved the co-existence of two phases, the presence side-by-side of particles of ice and supercooled water, the latter being much the more numerous.

At temperatures below 0°C, the vapour pressure in equilibrium with water exceeds that in equilibrium with ice at the same temperature; the magnitude of this difference vanishes at 0°C and reaches a maximum of about 0·26 mb near − 12°C. A cloud containing both ice and water at the same temperature is thus unstable; the water drops will evaporate and the ice crystals will grow by direct crystallization from the vapour. Bergeron estimated that, in the neighbourhood of − 12°C, significant growth of the ice particles would occur in a few minutes and that this was sufficiently rapid to explain the initial stage of growth to raindrop size. Once the ice particles have become large enough to fall through the cloud droplets at an appreciable rate, other processes, such as direct capture, may become significant and, indeed, in the later stages of growth, predominant. The speed of the initial process depends not only on the difference between the equilibrium vapour pressures and on the diffusive properties of water vapour, but also on the speed with which the growing ice crystal can lose the latent heat released at its surface, and the water droplet gain from its surroundings the heat lost by evaporation. A difference of temperature of 1°C between ice and water would suffice to stop the transfer completely. Detailed calculations of the rate of growth of ice crystals under such conditions have been made by Houghton (1950).

Bergeron summed up his theory in the statement ' almost every real raindrop and all snowflakes originated around an ice crystal.' The crucial step in the release of precipitation was thus claimed to be the appearance in the cloud of ice crystals; this was deemed to require that the top of the cloud should attain a sufficiently low temperature, considerably below 0°C, the ice being presumed to originate mainly by the freezing of cloud droplets although the possibility of direct crystallization from the vapour, on suitable nuclei, was also envisaged.

Findeisen (1938) developed the picture in more detail, maintaining that ice-particles could only form directly from the vapour phase on suitable ' sublimation ' nuclei; he too concluded that ' all appreciable formation of precipitation is initiated by sublimation.'

This view of the essential importance of the presence of ice in a raincloud became the working hypothesis of most meteorologists and there is indeed no doubt that most rainclouds in temperate latitudes do extend above the 0°C isotherm. There was never any justification for statements as sweeping as those in which these theories were summed up and the need for caution was expressed, for example, by Simpson (1941). Of recent years evidence has accumulated which shows conclusively that heavy rain can fall, under certain conditions, from clouds whose temperatures are nowhere below the freezing point.

Bowen (1950) and Ludlam (1951a), developing earlier ideas of Findeisen (1939) and Houghton (1938), have suggested that, in suitable circumstances, coalescence of droplets can provide the initial step in the growth of raindrops. This possibility was rejected by Bergeron (1933) who was contemplating a cloud of particles of uniform size. The new

picture starts from the assumption of the presence, near the cloud base, of a few particles appreciably bigger than the main population of cloud droplets and the occurrence of a significant updraught through a considerable thickness of cloud. The larger droplets are considered by Bowen to originate in the occasional coalescence of two cloud droplets of slightly different sizes. Ludlam suggests that, in maritime air masses, they may grow from the rare, very large, sea-salt nuclei. A larger droplet, whatever its source, will be carried up in the ascending air, although falling relative to the ordinary cloud droplets, some of which will be captured. The drop thus grows and, ultimately, if the updraught extends through a sufficient height, the rate of fall will exceed the speed of the updraught and the drop will return to the base of the cloud, still growing as it descends. Fig. 1 (after Bowen) shows the result of a computation of the trajectory of a drop growing in this way as it traverses a cloud with an updraught of 1 m/sec, average droplet diameter 20 μ and cloud water content of 1 g/m^3. The initial volume of the drop is taken to be twice that of the cloud particles. It is clear that this process could produce raindrops, given a supply of suitable initial droplets, a sufficient updraught and a cloud of considerable thickness. The last proviso will usually mean in practice, in temperate latitudes, that the top of the cloud will extend above the 0°C isotherm and may contain ice. It may thus be a difficult matter to decide which of the two possible processes caused the initial growth; both may be active.

This review will be largely concerned with a survey of the problems which have arisen in the detailed development of the ideas which have just been outlined and with the comparison of the observed properties of rainclouds with theoretical predictions. In order to avoid ambiguity it is convenient, here, to define certain terms which will be used frequently. *Condensation* will signify the deposition of liquid water from the vapour, *sublimation* the deposition of crystalline ice from the vapour, *coalescence* the union of two drops, *accretion* the growth of an ice particle by collision with water drops and *aggregation* the collision and adherence of two or more pre-existing ice crystals.

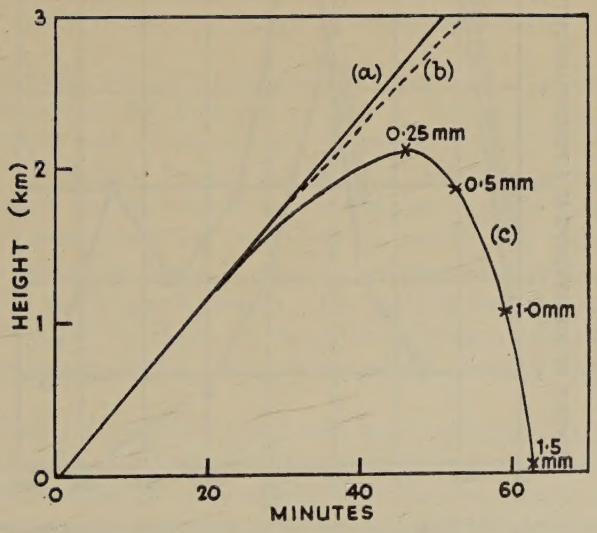


Figure 1. The trajectory of a drop growing by coalescence in a cloud with an updraught of 1 m/sec, average droplet diameter 20 μ and cloud water content 1 g/m^3 (after Bowen). Curve (a) represents the path of the air, (b) an average cloud droplet, (c) a droplet starting with twice the average volume. The positions of the growing drop when it attains diameters of 0·25, 0·5, 1·0 and 1·5 mm respectively are indicated on curve (c).

In the past few years great interest has arisen in the possibility of modifying the physical state of a cloud by artificial means and, in particular, of influencing the amount of precipitation or the time or place at which it occurs. Attempts have been made to accelerate ice formation by introducing fragments of solid carbon dioxide (' dry ice '), which causes intense localized cooling, or by introducing suitable freezing nuclei, silver iodide in particular. Alternatively, attempts have been made to promote the coalescence process by introducing hygroscopic nuclei (salt, for example), or even a spray of fine water drops, into a suitable part of the cloud. Numerous attempts at ' seeding ' clouds have been made from aircraft, balloons and even from the ground. The proper design, control and assessment of such experiments is not easy nor is our understanding of all the processes involved sufficiently detailed and precise to be of much assistance in arriving at a just verdict. Spectacular effects have occurred on some occasions but, despite the claims which have been made in some quarters, the general consensus of opinion is that the possibility of influencing rainfall by such methods on a large scale, sufficient, for example, to be of economic importance, remains unproven. Valuable recent summaries of the state of this subject have been published by the World Meteorological Organization (1954) and by Absalom (1954).

2. Observational evidence

2.1 *The growth of precipitation particles from ice crystals*

Bergeron (1933) and Findeisen (1939), although differing considerably in their detailed picture of the processes involved in the appearance of ice crystals in a cloud, were in

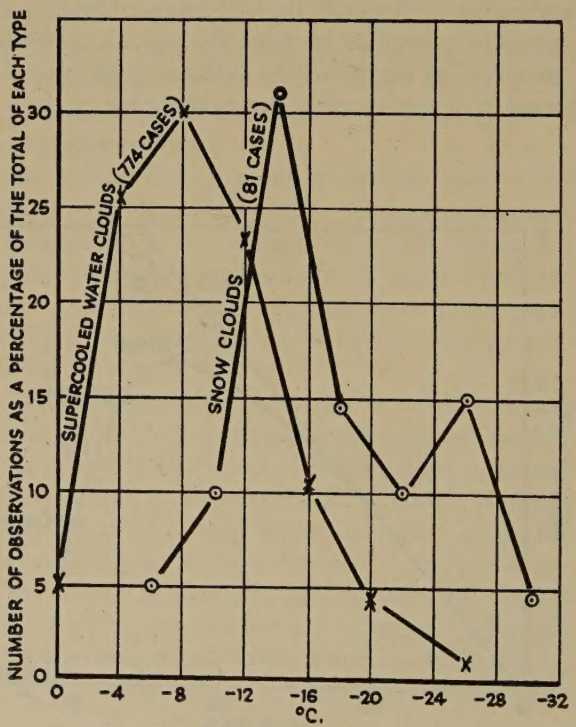


Figure 2. Graph of the results of Peppler's analysis of the cloud observations reported by German weather flights 1931-1935. The abscissa represents the temperatures of the cloud summits.

⊙ ' Snow clouds ': in which the observer reported that individual snow flakes could be seen and where very little or no ice accretion occurred on the aircraft.

× ' Supercooled water clouds ': in which the observer reported that no ice crystals could be seen and where ice accretion occurred on the aircraft.

agreement in claiming that ' almost every real raindrop . . . originates around an ice crystal ' and that clouds formed entirely of water particles can only produce the small drops of drizzle. The picture of the essential process in the change from cloud droplets to raindrops, as the rapid growth of ice crystals, in an atmosphere nearly saturated with respect to water, came to be very generally accepted. While direct evidence for the dominance of the process in precipitation clouds was meagre, it was accepted *faute de mieux*; other possible processes seemed likely to be much too slow.

2.11 *The temperatures of cloud summits*

Several series of routine measurements of the air temperature at the tops of clouds have been analysed by Peppler (1940), Stickley (1940) and by Mason and Howorth (1952). These seem to show that, in temperate latitudes, most heavy rain falls from clouds which extend several thousand feet above the freezing level. Some of Peppler's results are plotted in Fig. 2; although the heights attained by the aircraft in many flights were not very great the analysis extended over four years and was very thorough. Mason and Howorth analysed soundings by aircraft from Aldergrove, Northern Ireland. In 99 cases of rain for which cloud-top temperatures were known, these were below — 5°C on 87 occasions. On the other hand, in 45 examples of drizzle, the cloud-top temperature was above — 5°C in 39 cases. Stickley reported that, of 312 cases of precipitating cloud in the southern U.S.A., the temperature of the cloud top was below 0°C on 292 occasions, while on 10 occasions when the cloud-top temperature was above 0°C, only drizzle was observed.

These analyses refer only to temperate latitudes and the lack of observations of tropical clouds may account for the initial easy acceptance of the Bergeron theory. Such results do not of course prove that the process occurs but establish the existence of one condition necessary for it to be possible.

2.12 *Radar observations and cloud seeding experiments*

In the last ten years the two new techniques of radar and of cloud seeding have provided strong evidence that the Bergeron process is frequently operative. Radar examinations of rainclouds have often revealed a marked intensification of the echo from a level near the 0°C isotherm. Hooper and Kippax (1950a) interpreted this ' melting band ' as a clear indication that snowflakes were melting as they fell through this level. This interpretation has been confirmed by combined aircraft and radar observations (Bowen 1951) and the technique could be used to provide statistics of the frequency of rainfall associated with the Bergeron process. The melting band provides particularly striking evidence in favour of the Bergeron theory as its presence implies the absence of the strong updraughts (exceeding 1 m/sec) necessary to the Bowen-Ludlam coalescence process (cf. Section 5).

The effects of seeding a supercooled cloud with many ice crystals, produced by the low temperature of solid carbon dioxide, were observed by Kraus and Squires (1947), and the experiment has been repeated on many subsequent occasions, other nucleating agents such as silver iodide being sometimes used. Where appreciable amounts of rainfall, or a substantial modification of the cloud, are produced, the experiments give clear evidence of the operation of the Bergeron process. Such cases seem to be a small percentage of the attempted experiments and most of the work in this field has been characterized by a lack of careful analysis.

2.2 *The growth of precipitation particles by coalescence*

Well-documented observations of ' non-freezing rain ' (rain of considerable intensity from clouds throughout which the temperature exceeded 0°C) have been reported with increasing frequency since the war. Kotsch (1947) and Virgo (1950), for example, describe typical examples of non-freezing showers, observed from aircraft, while Mordy and Eber (1954) report many examples of moderate and heavy rain from warm clouds (cloud temperature everywhere exceeding + 7°C) on an island in the Hawaiian group, together with details of rainfall rate, drop-size distribution, cloud depth and orographic effects. E. J. Smith (1951a, b) discusses several simultaneous aircraft and radar observations of non-freezing rain over New South Wales, in great detail. These papers have established, beyond doubt, the occurrence of rain from warm clouds. The examples they discuss are all, however, in warm or tropical maritime air and the question arises as to the frequency of occurrence of this type of rain deep inland and in cooler climates. The position here is unsatisfactory though various observations suggest that, in England, non-freezing rain is rare. Jones (1951) reports a single case of slight rain from a line of warm cumulus near London. Indirect evidence in favour of the occurrence of non-freezing rain in England has been given by L. G. Smith (1951) from a consideration of the association of heavy rain with strong electric fields. Usually clouds which produce rainfall rates exceeding about 15 mm/hr, produce intense electric fields (greater than 1,000 v/m) at the ground. These fields are generally thought to be associated with the presence of ice in the cloud. From an analysis of some observations taken over two years by Simpson at Kew, Smith finds that on 7 occasions, intense rain was associated with only weak electric fields. On 6 of these occasions, the temperature at the level of the cloud top, estimated from a tephigram, was above − 3°C, whereas on the numerous occasions on which intense rain was associated with strong electric fields the estimated cloud-top temperature was always below − 10°C. This evidence is extremely suggestive.

There are no published observations of non-freezing rain in continental air masses. Byers (1949) has pointed out that this may be due to the absence of the giant sea-salt nuclei which serve to form the large droplets necessary to initiate the Bowen-Ludlam process.

The occurrence of rain from warm convective clouds is strong but not conclusive evidence for the importance of the Bowen-Ludlam process. That this process is mainly responsible for non-freezing rain is indicated by some other lines of evidence. E. J. Smith, in his observations of clouds giving non-freezing rain, found that the rain-water content increased sharply towards the top of the cloud, a fact supported by some radar observations discussed by Bowen (1950). This level of high rain-water content may be tentatively identified with that at which the terminal velocities of the growing drops are just equal to the velocity of the updraught which has carried them aloft, and from which they subsequently fall as rain, in accordance with the Bowen-Ludlam theory (cf. Fig. 1).

Bowen (1952) has claimed successful stimulation of artificial rain on 4 out of 10 attempts by the injection of water droplets into the base of growing cloud. Rain, or rain and hail, fell within 40 min. The occurrence of hail on two of the successful attempts confuses the results, in that it might be considered evidence in favour of the Bergeron process !

It will be difficult to evaluate the importance of the Bowen-Ludlam process in temperate climates where the freezing level is usually below 3 km, because clouds deep enough to form rain by this process will usually extend above the freezing level. In such cases, it is impossible to distinguish between the Bergeron and Bowen-Ludlam processes by temperature measurements (cf. Section 4).

3. Precipitation elements

3.1 Physical properties of precipitation elements

3.11 *Terminal velocities*

In order to study the processes occurring in rainclouds it is important to know the velocities of fall of raindrops, hailstones, snowflakes and ice crystals.

If a particle is released from rest in still air, it will accelerate to a terminal velocity which is reached effectively, by a raindrop, after falling about ten metres. The terminal velocity is determined by the mass, size and shape of the particle and by the density and viscosity of the air, and it thus varies with height. Best (1950a) has reviewed various measurements and gives an empirical formula for the velocity of a waterdrop at any height; the terminal velocities, at heights of 3 km and 6 km, are found to exceed those near the ground by about 10 per cent and 30 per cent respectively. The most recent and consistent measurements are those of Gunn and Kinzer (1949). It should be noted that Stokes's Law is applicable only for diameters less than about 50 μ, the drag due to the wake becoming important for larger drops. This drag is considerably enhanced by the distortion of drops larger than 3 mm in diameter.

Few data are available on the terminal velocities of hailstones. Weickmann (1953) maintains that they have poor aerodynamic properties and a drag coefficient considerably greater than that of ideal spheres; he suggests an approximate formula for the velocity, v cm/sec,

$$v = 200\, D^{\frac{1}{2}}, \text{ where } D \text{ is the diameter in mm.}$$

This implies that some earlier workers (e.g., Bilham and Relf 1937) have considerably overestimated the rates of fall of large hailstones.

There have been a number of attempts to measure the terminal velocities of snowflakes, a problem beset with practical difficulties. The recent results of Langleben (1954) are systematic and remarkably consistent, showing in a useful manner how the terminal velocity depends on the form and size of the flake. As a rough rule, it can be stated that ice crystals with diameters from 300 μ to 1,000 μ fall with velocities between 0·4 and 1·0 m/sec, while large dry snowflakes fall at about 1·5 to 2·0 m/sec.

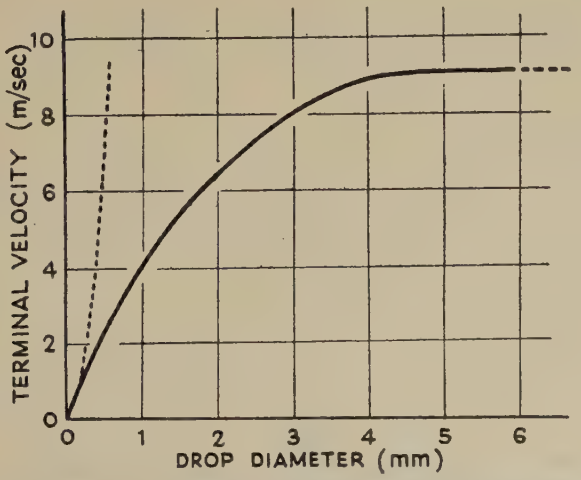


Figure 3. The terminal velocities of freely falling raindrops in air at N.T.P. (after Best 1950a). The pecked line shows the terminal velocities that would be attained if Stokes's Law were obeyed for all sizes of drop.

3.12 *Collection efficiencies*

The rate at which a drop, of mass m and diameter D, grows in falling a distance z relative to the air in a homogeneous cloud of droplets of diameter δ and of liquid-water content w, is given by the equation :

$$\frac{dm}{dz} = E\,(D,\,\delta)\,\pi\,D^2\,w/4$$

The factor $E\,(D,\,\delta)$ is called the collection efficiency; it lies usually between 0 and 1 and represents the fact that some of the droplets lying in the path of the falling drop are swept around it and escape collision. The equation assumes that all collisions lead to coalescence, an assumption to be examined in the next section. The collection efficiency varies in a complex way with the diameters of the drop and droplets, as represented in Fig. 4 after Langmuir (1948). A droplet tends to follow the airflow around a falling drop, the deflecting force depending on the viscosity of the air. The form of the airflow and the velocity near the surface depend on the diameter of the drop; the ratio, of the inertia of a droplet to the viscous force acting on it, increases with droplet size and thus, for a given D, E increases with δ.

The main difficulty in computing values of collection efficiency concerns the exact form of the airflow around a falling particle. Langmuir (1948) assumed viscous flow for Reynolds numbers less than 60 and potential flow for greater values of Reynolds number. He further assumed that collision would occur for all droplets whose centres passed within a distance $\frac{1}{2}D$ of the centre of the drop. The problem is very difficult when D and δ become comparable. Ludlam (1951a) has suggested that, in this case, a better approximation to the truth is obtained by increasing the collection efficiencies given by Langmuir by a geometrical factor $\{1 + \delta/(DE^{\frac{1}{2}})\}^2$.

There has been no satisfactory experimental check on Langmuir's values of collection efficiency for drops, although values obtained by a similar method for cylinders have been verified. Measurements by Gunn and Hitschfeld (1951) of the growth, by coalescence, of drops (diameter 3·2 mm) falling through an artificial cloud, containing droplets with diameters from 5 to 50 μ, are consistent with Langmuir's values. Observations on natural clouds by Adderley (1953), on the other hand, suggest, if his results are taken at their face value, that the relative values of Langmuir's collection efficiencies, for drops of 0·5 and 1·0 mm diameter respectively, are wrong by a considerable factor. There is great uncertainty in the values of the collection efficiencies of small drops such as would be important in the initial formation of rain by a coalescence process.

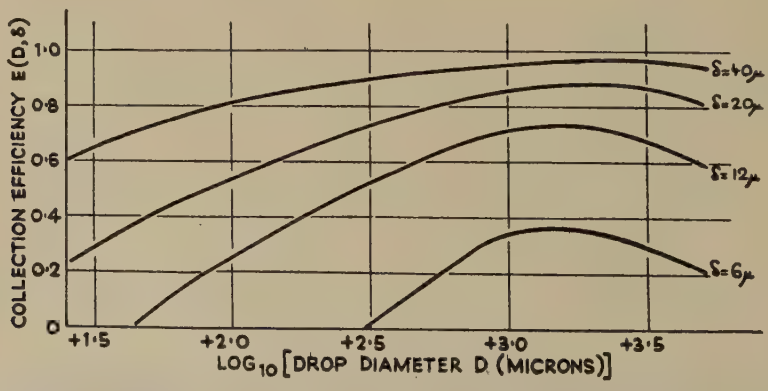


Figure 4. The collection efficiency $E\,(D,\,\delta)$ for a drop of diameter D (microns) falling at its terminal velocity in air at N.T.P. through a homogeneous cloud of water droplets of diameter δ (microns) (after Langmuir 1948).

Still less is known of the collection efficiencies of plate and needle-like ice crystals for droplets, and no computations are available. It is not even certain whether viscous or potential flow should be assumed for small Reynolds number (e.g., see Browne, Wexler 1953), yet calculations assuming viscous flow lead to values of collection efficiency roughly one-fifth of those obtained by the assumption of potential flow. In view of the importance of accretion processes, this is a most unsatisfactory state of affairs. Further knowledge will not come easily and all calculations must certainly be checked carefully by experiments.

3.13 *Coalescence of drops*

The total surface energy of two drops is greater than that of the composite drop formed by coalescence, so that energy is released by the process. Coalescence may conceivably occur as soon as two freely-falling drops touch, or it may occur only after the surfaces in contact have been flattened slightly. If the latter is the case, a certain amount of energy must be provided for the initial deformation with its consequent increase of surface area. In the former case, it is not obvious whether or not such a ' potential barrier ' must be surmounted before coalescence can occur – there will be no potential barrier only if the drops coalesce in such a way that their total surface area decreases steadily throughout. Although no theoretical work on the problem has appeared, there has been considerable experiment to determine the conditions under which coalescence can occur.

Rayleigh (1879, 1882) observed that drops of diameter 1-2 mm, formed from unstable jets of water, coalesced upon collision only in the presence of a weak electric field. His conclusion that coalescence will not normally occur is apparently supported by the observations of Dady (1947) that no coalescence occurred in several thousand collisions between oil droplets 4 μ in diameter. The use of an atomiser to produce the droplets in these experiments may, however, have given to them electric charges of like sign, leading to mutual electrostatic repulsion.

Diametrically opposite conclusions were reached by Dessens (1949) and Gabilly (1949). In Dessens's experiments, droplets of diameter about 20 μ, attached to fine spider threads, were brought gradually together, and coalescence occurred always at contact. Gabilly allowed droplets suspended in an airstream to strike others attached to fine threads. Both these experiments can be criticized on the grounds that a droplet fixed to a support is distorted, so that cloud conditions are not simulated.

Indirect evidence that coalescence always occurs on collison between large drops and small droplets in a cloud was obtained by Gunn and Hitschfeld (1951) in the experiments discussed in the previous section.

It can be seen that the experimental results are apparently conflicting. The effects of surface impurities and electric fields may be important and more carefully controlled experiments would be welcome. Some indication of the results that might be expected is given by the following ' order-of-magnitude ' reasoning. The surface energy S of a drop of radius r and surface tension γ dynes/cm is given by

$$S = 4\pi r^2 \gamma$$

The potential barrier to coalescence, if it exists at all, can be expected to depend on S and we may write

$$\Delta S \geqslant C\, 4\pi r^2 \gamma$$

where r is taken as the radius of the smaller of the two colliding drops and C is a dimensionless constant. The energy available for surmounting the potential barrier ΔS will be the

difference between the total kinetic energies before and after coalescence. If the radius of the large drop is much greater than that of the small drop, then the kinetic energy available for overcoming the potential barrier is given by $\frac{1}{2} mU^2$, since the collision is inelastic, where m is the mass of the smaller drop and U the velocity of the larger drop. Since $m = \frac{4}{3}\pi\rho r^3$, the condition that coalescence should occur reduces to

$$r U^2 > 6 C\gamma/\rho$$

Putting $\gamma = 80$ dynes/cm, $\rho = 1$ gm/cm^3 and $C = 10^{-2}$ (Browne 1952a), this reduces to $r U^2 > 4{\cdot}8$ cm^3 sec^{-2}. Thus, in considering collisions between raindrops and cloud droplets, as in the experiments of Gunn and Hitschfeld, we put $U = 300$ cm/sec, and find that coalescence will occur if the radius r of the cloud droplet is greater than $0{\cdot}5$ μ. On the other hand, the argument indicates that a cloud droplet of radius 4 μ will coalesce only with drops of radius greater than 150 μ, a result consistent with Dady's results. The kinetic energy available for coalescence may be increased in turbulent air and the minimum droplet radius required for coalescence reduced. In conclusion, we must regard the problem of coalescence as still undecided, even though the tacit assumption that coalescence always occurs on collision has so far led to no inconsistencies, and has enabled plausible theories of rain formation to be put forward.

3.14 *The fragmentation of drops and splintering of ice crystals*

Lenard (1904) observed that freely falling drops larger than 5·5 mm in diameter were unstable, and disintegrated. This fact is important as it provides a mechanism for ' chain reaction ' rain formation (see Section 5).

Blanchard (1948) has made a recent study of the instability of drops and finds that in calm air, drops can reach a diameter of 7 mm before becoming unstable, but the slightest turbulence reduces the diameter of the greatest stable drop to 5 mm. Blanchard observed that an unstable drop broke up into two to ten large fragments together with numerous droplets. The results were little affected when the surface tension of water was reduced by contamination from 80 to 56 dyne/cm.

Although he observed that the shock of collision with a cloud droplet would sometimes disrupt a drop, Blanchard did not study the effects of shock produced by sudden

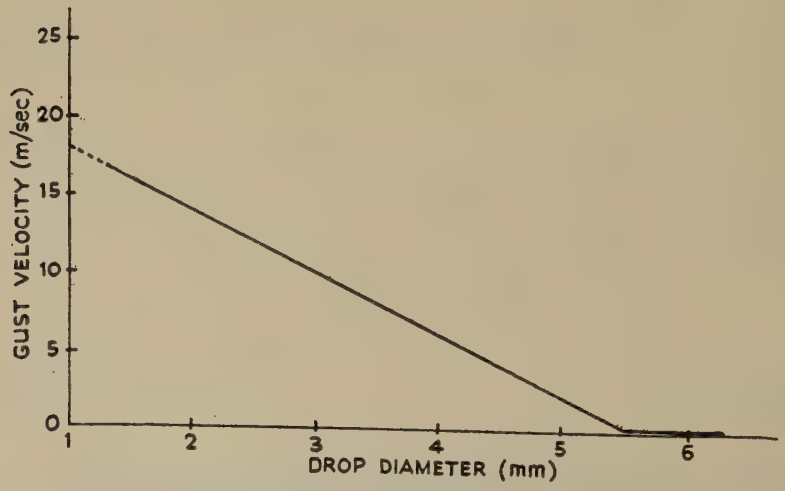


Figure 5. Graph showing the velocity of a sudden gust sufficient to disrupt a water drop falling at its terminal velocity through air at N.T.P. (after Hochschwender 1919).

violent gusts of air. Information on this matter is provided by some experiments of Hochschwender (1919), who gives a table showing, for various drop sizes, the minimum velocity of a drop, relative to the air, needed for disruption. By subtracting from this the terminal velocity of the drop, the graph above (Fig. 5) is obtained, giving the minimum sharp-edged gust velocity needed for disruption.

The splintering of ice crystals growing by accretion or sublimation has been observed by Findeisen (1943) and by Palmer (1950) who suggested that the process may provide ice-crystal nuclei greatly outnumbering the naturally occurring freezing nuclei (§ 3.22). Splintering seems to occur most readily from dendritic crystals, and for this reason it is difficult to assess its importance, because such crystals are rare in natural clouds at temperatures below — 5°C.

3.2 Growth processes

3.21 *Condensation nuclei*

It is generally agreed that all natural cloud droplets formed from the vapour commence their growth around ' condensation nuclei.' Since the nature and concentration (number per unit volume) of the nuclei control the *initial* size distribution of cloud droplets, their determination is important.

As the relative humidity in cloud-free air is increased by cooling, condensation will occur first on the larger nuclei, other things being equal. If the cooling is sufficiently gradual, the initial droplets will be able to take up all the available water vapour, but if there is rapid cooling, the relative humidity may continue to rise, and condensation will commence on successively smaller nuclei. When different kinds of nuclei are present, condensation will favour the more hygroscopic ones. Wright (1939) gives the size of nucleus required for droplet formation at various relative humidities, on the assumption that the nuclei are minute droplets of common-salt solution of varying strengths.

Many estimates have been made of the concentration of condensation nuclei in the atmosphere, and a brief summary of the results is given below, taken from Simpson (1941) and Wigand (1919) :

	Mean values of nucleus concentration (nuclei/cm^3)
Upper air (10 km height)	2
Sea air	950
Inland air away from towns	9,500
City air	150,000

Since these nucleus counts were made with Aitken counters they may be misleading. For example, sea air may contain relatively few hygroscopic nuclei while inland air may contain numerous small non-hygroscopic nuclei. Copious cloud could form in the sea air, with a slow rate of cooling, at lower relative humidity than over land, although the sea air would give a lower Aitken count !

Results of more interest are obtained from correlating weather conditions with nucleus counts. Moore (1952) and Woodcock (1950) have made nucleus counts at sea, determining total number with Aitken counters and measuring the size distribution of nuclei more massive than 10^{-11} g by impactor techniques. From an analysis of such results, and from observations of the reduction in visibility caused by the larger hygroscopic nuclei (Wright 1939), Moore concludes that the number of large nuclei increases markedly with wave height and wind speed, and is able to explain why previous investigators had failed to detect this correlation. These results suggest that the larger nuclei in maritime air at sea level are almost entirely due to evaporated sea-spray droplets, a result supported by Moore's observations that the nucleus concentration at sea level falls when there is

vertical convective mixing in the air. Even inland, a large proportion of the larger (and therefore more important nuclei) appear to consist of sodium chloride (Dessens 1949). Dessens estimates that, inland in France, the average concentration of salt nuclei of radii greater than 0·1 μ is about 100 cm^{-3} and with radii greater than 1 μ of the order of 1 cm^{-3}. Even the latter figure is great enough to be important in the initiation of rain by the Bowen-Ludlam process.

3.22 *Ice-forming nuclei*

It was long assumed that atmospheric ice crystals were formed directly on nuclei in a manner analogous to the condensation of water drops. Wegener (1910, 1920) suggested that such ' sublimation nuclei ' would be effective as soon as the air became saturated with respect to ice. The matter was not investigated further until Krastanow (1941) showed that the formation of ice crystals in pure water vapour is an inherently improbable process except at temperatures below — 70°C. At higher temperatures ice crystals are not normally formed until water saturation is reached, and then probably by the freezing of liquid water. The work in this field has been reviewed by Mason and Ludlam (1950).

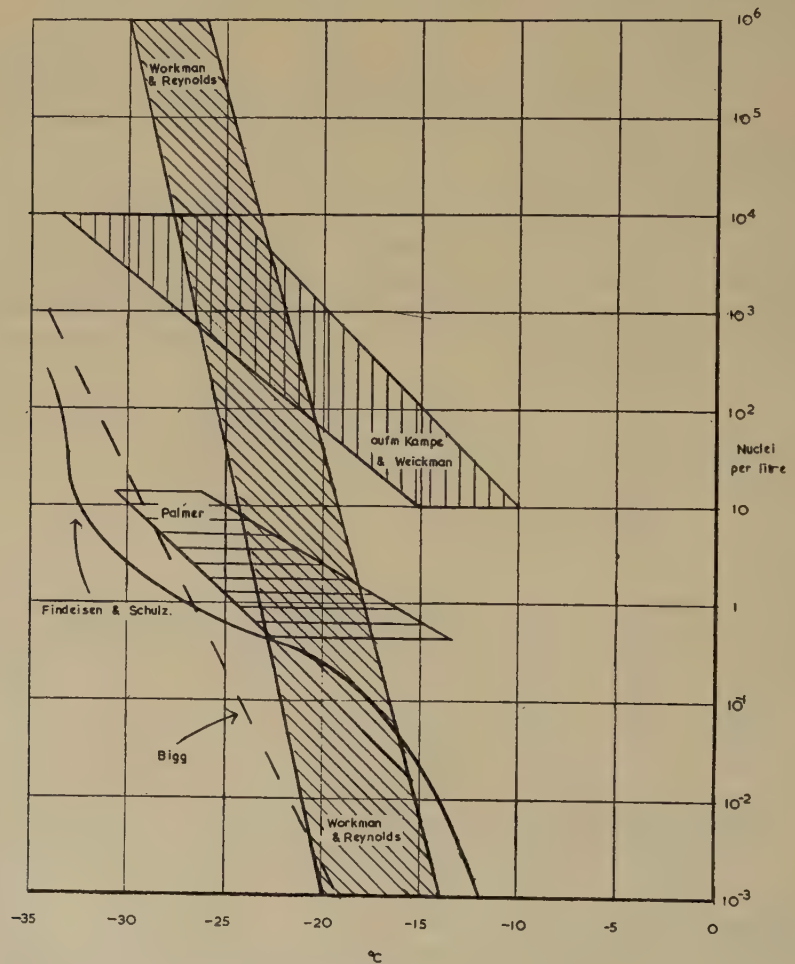


Figure 6. The spectra of freezing nuclei observed by different workers (after Aufm Kampe and Weickmann 1951). Bigg calculated the spectrum shown from his observations of the supercooling of distilled water (Bigg 1953a, b). He assumed a rate of ascent of 20 m/sec.

Findeisen and Schulz (1944) reported some careful measurements of the concentration of ice-forming nuclei, using a large chamber of volume 2 m^3 in which clouds could be produced by adiabatic expansion. The large size of the chamber allowed the use of very slow rates of expansion, corresponding to updraughts in natural clouds of 5 m/sec. They assumed that each ice crystal seen had been formed on a freezing nucleus, and published the well-known ' freezing-nucleus spectrum ' reproduced in Fig. 6. There have been several investigations of atmospheric air by an expansion technique (e.g., Cwilong 1947, Palmer 1949); the conditions, in these experiments, become similar to those in natural clouds as the size of the cloud chamber is increased and, on this account, the experiments of Findeisen and Schulz are probably the most satisfactory.

In a second group of experiments clouds have been produced by mixing a little warm moist air with a large volume of cold saturated air, frequently by the expedient of breathing into a cold tank. Large cold rooms have been used, 10 m^3 by Workman and Reynolds (1949a), 30 m^3 by Aufm Kampe and Weickmann (1951), for some of these experiments and the large volume has sometimes been thought to provide a closer approach to atmospheric conditions. It is clear, however, that the individual cloud droplets, and freezing nuclei if present, are cooled much more rapidly in the formation of such a mixing cloud than in any natural cloud, and the results of such experiments are relevant to atmospheric processes only if the rate of cooling does not influence the temperature at which ice crystals appear. Findeisen used different rates of adiabatic cooling, all much slower than the rate at which the temperature of the nuclei must fall in a mixing cloud, and for this slow cooling he found more ice crystals for lower rates of expansion. This trend was reversed for rates of cooling corresponding to updraughts exceeding 50 m/sec. Bigg (1953a) has also found that the freezing temperatures of water drops fall slightly at higher rates of cooling (*vide infra*). Simultaneous measurements, using the two techniques, of the nucleus spectrum at one place should elucidate the importance of this effect.

The interest of the freezing-nucleus concept led to the neglect of an alternative source of ice crystals, the freezing of fully-grown cloud droplets. As it is impossible to follow the history of a freely-falling cloud drop, we have no direct knowledge of the temperatures at which they freeze, but there have been many observations of the freezing of drops supported on cooled surfaces. The earlier work, which has been reviewed by Mason and Ludlam, did not greatly help our understanding of ice formation in the atmosphere. Recent work by Heverley (1949), Dorsch and Hacker (1950) and Brewer and Palmer (1951) suggests that, in general, smaller drops freeze at lower temperatures and this effect has been more fully investigated by Bigg (1953a, b). In order to reduce the effects of surface contamination he observed the freezing of drops supported between two immiscible liquids. He concluded that the probability P of freezing depended only on T_s, the degree of supercooling below 0°C, the volume V of the drop and the time t for which it had been kept at the lower temperature : his results could be represented by the equation

$$VtK \exp(aT_s - 1) = -\ln(1 - P) \simeq +P \text{ when } P \ll 1$$

where $a \simeq 1$ and K is of order 10^{-8}. Bigg established that these results could be reproduced with distilled water from various sources and with different supporting liquids; suggesting that the equation would also hold for cloud drops, he has re-interpreted Findeisen's results. With assumptions concerning the humidity of air in the chamber and the concentrations of condensation nuclei, he was able to calculate the size and concentration of the cloud droplets and hence the concentration of ice crystals which should be observed under differing conditions. At temperatures below about − 20°C the values deduced agreed well with Findeisen's measurements, but at higher temperatures Findeisen's crystals were the more numerous by a factor of about 10.

Bigg's results are of considerable interest, although, as he emphasized, they do not tell us anything about the physical process of freezing. The results of cloud-chamber experiments on a single day could be explained either in terms of the properties of freezing nuclei or by the freezing of drops. If, however, the nucleus spectra for several days are compared, there are frequently discrepancies of 5-10°C between the temperatures at which some particular concentration of ice crystals was observed. These might be explained by variations in the concentration of *condensation* nuclei, which would cause similar expansions to produce droplets of different sizes. It would thus be a valuable experiment to repeat cloud-chamber measurements, the samples having first been diluted with cleaned air to standard condensation-nucleus concentration. If the variation of the spectrum were still observed, the constants in Bigg's equations would require daily adjustment and this would seem to suggest that the freezing of the cloud drops was brought about by variable, presumably foreign, particles and one would be very close to a freezing-nucleus theory.

3.23 *The nature of freezing nuclei*

A certain amount of work has been carried out to determine the nature of freezing nuclei, with the primary object of detecting geographical influences. Schaefer (1951) finds that dust varies in its efficacy with its source, but the variation is not strikingly large, while Vonnegut (private discussion) has suggested that atmospheric pollution may inactivate many nuclei formed near the ground. Most experiments to detect ice-forming nuclei have been performed at the earth's surface, usually in populous industrial districts. Even so, the results shown in Fig. 6 do not agree well, and there has been no careful investigation of the variation of the nucleus spectrum with geographical position, with origin of the local air mass, or with height in the atmosphere. E. J. Smith and Heffernan (1954) have carried in an aircraft a chamber of 76-litre capacity in which clouds were formed by mixing. They found that, in the range of height 1-3·5 km, the concentration of ice crystals in the cloud chamber tended to be greater above an inversion but otherwise showed little correlation with height.

Considerable interest has been aroused by a recent suggestion (Bowen 1953) that meteoric dust particles may act as extremely efficient freezing nuclei. Bowen finds that the rainfall, averaged over fifty years at several stations in the southern hemisphere, shows peaks on certain dates about 29 days after some of the major annual meteor showers. He considers that these peaks are real, and not caused by statistical fluctuations. If this is accepted, and the point will of course demand a very searching scrutiny, it would be hard to escape the conclusion that there is a ' cosmic ' influence acting on the rainfall. It is believed on experimental and theoretical grounds that, while meteor particles more than a few microns in radius (mass greater than 10^{-11} g) evaporate completely in their passage through the upper atmosphere to give heat, light and ionization, smaller particles are unscathed and will fall to the tropopause in about one month or longer. Bowen suggests that it is these ' micrometeorites ' which can act as freezing nuclei. The theory is attractive, but certain objections can be raised on astronomical grounds. It is believed (Kaiser 1953) from the evidence of radio echo and visual observations, and from certain theoretical considerations, that for meteors fainter than 5th magnitude, the diurnal flux of sporadic meteors incident on the earth greatly exceeds the maximum diurnal flux during any of the major annual showers (with the possible exception of the Arietid daytime shower). If this law holds down to the micrometeorites, which there is no reason to doubt, then the additional flux of micrometeorites produced during a meteor shower would not be noticeable. To overcome this difficulty, it could be argued that freezing nuclei are produced by the flaking of the brighter larger visual meteors, which

outnumber the comparable sporadic meteors considerably during meteor showers. There is little evidence for such flaking, except possibly for meteors brighter than third magnitude (Ceplecha 1953) but even if we assume that such meteors are completely pulverised into fragments of mass 10^{-11} g spread along the path of the meteor (10 km or more), it can be shown that the concentration of the particles near the earth's surface will be less than $0{\cdot}1\ m^{-3}$. This seems insufficient to affect markedly the rate of freezing of supercooled droplets in a cloud. However, it is clear that Bowen's suggestion needs more than a superficial examination before a conclusion is reached, and his observations, if they are valid, will be hard to explain by any other theory.

3.24 *Condensation, evaporation and sublimation*

Jeffreys (1918) showed that when the growth of a particle is governed by the diffusion towards it of vapour the rate of growth is given by the equation

$$\frac{dm}{dt} = 4\pi\Delta\, CM\,(p - p_s)/\mathbf{R}T$$

where m is the mass of the particle, C is the electrostatic capacity of a conductor of the same shape and size, Δ is the diffusion coefficient for water vapour in air, p_s and p are the water-vapour pressures at the surface of the particle and at a remote point in the vapour respectively; M, $\mathbf{R}$, T are molecular weight of water vapour, gas constant per mole and absolute temperature respectively. In the case of spherical drops C is equal to the radius. Frössling (1938) pointed out that the motion of the drop through the air would increase the supply of vapour and, to allow for his effect, multiplied the right-hand side of the equation by a wind factor f. His experimental results indicated that f varied with the Reynolds number Re of the flow round the drop ($f = 1 + b\,(\mathrm{Re})^{\frac{1}{2}}$; b is approximately 0·3). These results were confirmed for the evaporation of freely-falling drops by Kinzer and Gunn (1951).

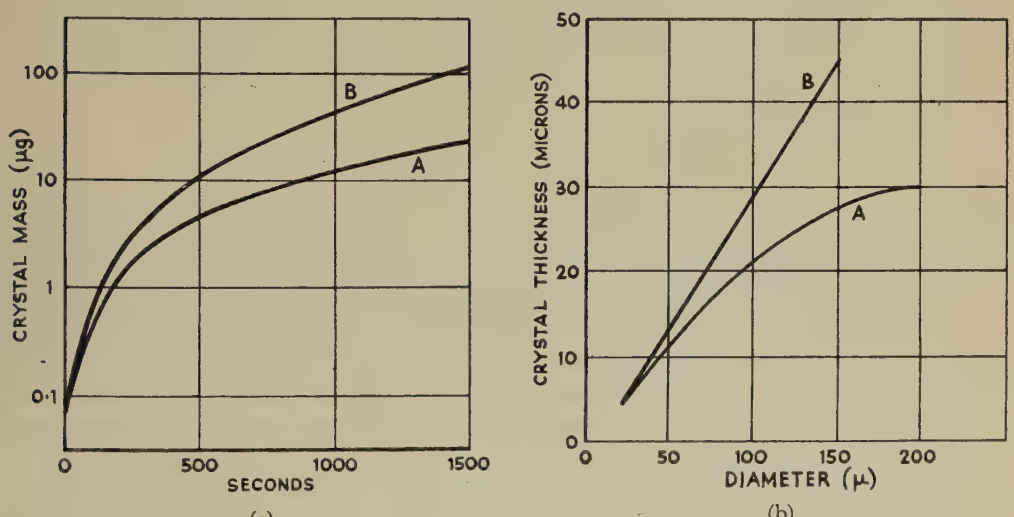


Figure 7. (a) The growth of ice crystals in a water-saturated atmosphere at − 10°C (the wind factor was assumed constant = 2·5). Curve A shows the growth if the crystal thickness is equal to one-fifth of the radius, and curve B shows the growth if the thickness is constant = 10 μ. The crystals are initially identical with a mass of $6{\cdot}25 \times 10^{-8}$ μg and circular shape. (b) Reynolds's measurements of the thickness of ice crystals grown at about − 18°C. Curve A, plane dendritic crystals; curve B, hexagonal plate crystals.

The value of p_s may be adjusted to allow for the effects of the curvature of the drop and for the heating or cooling produced by the latent heat of vaporization; the drop will usually be at the ventilated wet-bulb temperature (Kinzer and Gunn 1951). If the drops contain hygroscopic substances the change in p_s may, with caution, be deduced from Raoult's law; the numerous pitfalls in such a calculation have been emphasized by Macdonald (1953).

The growth of ice crystals by sublimation has been discussed by Houghton (1950) and Mason (1953); they replace C in the equation above by the capacity of a conductor of an appropriate simple geometrical shape. The equation does not predict how the crystal will grow but only the instantaneous rate of growth for a particular size and shape. Fig. 7a shows the effect of different assumptions about the thickness of plate crystals. Mason (1953) and Reynolds (1952) have measured the rate of growth of small ice crystals in supercooled water clouds but their crystals were too small to show in Fig. 7a. Reynolds also observed the relation between the radius and the thickness of plate and dendrite crystals and his results are shown in Fig. 7b. Weickmann (1945) has reported observations of the forms of atmospheric ice crystals and Nakaya (1951) has reproduced many of these but, unfortunately, the conditions at the surface of the artifically grown crystals were not very well defined.

The growth of a dendritic crystal cannot be calculated from the equation above, since the value of C is unknown, although it might be found from model experiments. Mason has used dimensional arguments to estimate that hexagonal plates should commence dendritic growth when the diameter exceeds about 100 μ. Browne (1953) and Murgatroyd (private communication) report however that dendrites are rare in nimbostratus and hexagonal plates as large as 400 μ are common.

4. Physical properties of rainclouds

4.1 *Layer clouds*

The physical properties of rain-forming layer clouds will be discussed in an effort to present a general description of their large- and small-scale features. The discussion will be restricted to pure layer clouds, thus excluding systems of cumulonimbus embedded in nimbostratus. In England, nimbostratus usually gives a rainfall rate of from 1 to 3 mm/hr.

4.11 *Cloud depth and extent*

Clouds of this type often cover an area of thousands of square kilometres and are commonly associated with frontal systems. The depth of layer clouds and the height of the cloud base have been investigated by several workers (see § 2.11). Mason and Howorth (1952) give a rough rule relating the thickness of rain-forming stratus to the height of cloud base : clouds whose thickness exceeds the height of the cloud base by more than 1 km will precipitate. The data on which this statement is based were all obtained in one region, viz. Northern Ireland, and some caution should be used in generalizing from them. The fact that a cloud of given depth will precipitate only if its base is below a certain level presumably depends on the fact that a low cloud base tends to imply air of high humidity mixing ratio and a cloud of large liquid water content.

4.12 *Updraught and turbulence*

No direct measurements of updraught in layer cloud are available. This is not surprising; only a very small updraught can be expected. Bannon (1948) has estimated

updraughts from observations of the mean rainfall rate below layer cloud by the assumptions that the cloud is in a steady state and that the water falling from a vertical column comes entirely from the release of water in the vertical upcurrent. His calculations indicate that the updraught speed will vary from 5 cm/sec up to 20 cm/sec for rainfall rates in the range 1-3 mm/hr. Updraughts may also be estimated crudely from the convergence along trough lines in depressions (e.g., Hewson and Longley 1944); the values estimated in this way show fair agreement with those obtained by Bannon.

The turbulence in layer clouds is probably also slight. It presumably arises mainly from the vertical shear of the horizontal winds, and this is generally only of the order of 2×10^{-3} sec^{-1}. Few turbulence measurements have been reported. Browne (1952a) has estimated, from radar observations, the intensity of turbulence (ratio of mean square fluctuating velocity to mean wind speed) in warm frontal layer cloud as less than 0·1 per cent at a height of 3 km, but this value seems exceptionally small.

4.13 *Humidity*

It has generally been assumed that in nimbostratus the air is saturated with respect to water. This is doubtless justified below the freezing level, but may not be at greater heights where the presence of ice crystals introduces a complication. No direct measurements are available, because of their difficulty. The matter will be discussed at length in Section 5.

4.14 *Liquid-water content and droplet-size distributions*

There is a surprising lack of published systematic information on the liquid-water contents and droplet-size distributions in layer cloud, yet many measurements must have been made. Kline and Walker (1951) give liquid-water contents above the freezing level ranging from 0·05-1·3 g/m^3. Cunningham (1951) measured liquid-water contents, with a rate-of-icing meter, from 0·05-0·2 g/m^3 in clouds giving rainfall rates between 1 and 2 mm/hr, and from 0·1-0·2 g/m^3 in a layer cloud giving a rainfall rate of 7 mm/hr. Other indirect estimates were made by Browne (1952a) from absolute measurements of the radar-echo intensity below the freezing level in precipitating layer cloud. He finds liquid-water contents of less than 0·05 g/m^3 for three clouds giving rainfall rates from 0·9-1·1 mm/hr, and values of 0·40-0·55 g/m^3 for a cloud giving rainfall rates from 2·5-3·5 mm/hr.

Measurements of cloud-droplet size have been reported for non-precipitating layer cloud, in regions below the freezing level, by Frith (1951) who gives values of droplet-size distribution showing diameters up to 30 μ, with 50 per cent of the total water content contained in droplets of diameters between 12 μ and 18 μ. Kline and Walker (1951) find that the bulk of the water in supercooled layer clouds is contained in droplets with mean diameter 12 μ, the largest droplets having diameters of about 40 μ.

4.2 *Convective rainclouds*

Rain from convective cloud is characterized by its variability and short duration as compared with layer-cloud rain. In a moderate April shower in England, rain may fall for perhaps half-an-hour, and its maximum intensity may reach 10-15 mm/hr. In a heavy thunderstorm, rain may fall from a single 'cell' (see below) for as long as one hour, and its peak intensity, in this country, will usually be about 50-80 mm/hr, though higher values are not uncommon.

4.21 *Cumulonimbus cells*

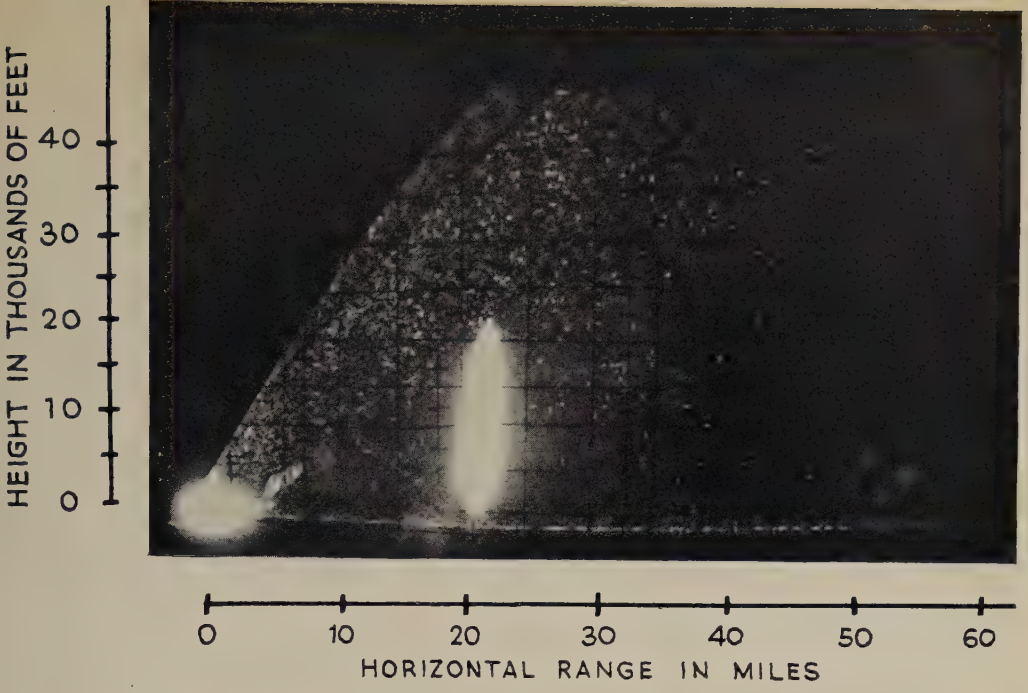
A great advance in the study of convective rain was made when it was realized that well-developed cumulonimbus clouds contained one or more ' cells,' each with a fairly definite life-cycle (*The Thunderstorm* 1949). The existence of these cells is clearly seen in numerous radar range-height photographs (Fig. 8). Initially, in the cumulus stage, a cell is growing and characterized by an updraught which lasts on the average for some 20 min. In the mature stage, precipitation is released, and the drag of the falling drops (which is equal to their weight) initiates a downdraught which spreads downwind through the cell, shutting off the further supply of moist air. In the final dissipating stage, the cell subsides and evaporates slowly, often to be absorbed by another cell. A large thundercloud may contain, at any instant, cells in all stages. Each cell converts into precipitation about 10^7 to 10^8 kg of water (Braham 1952). It has been observed that the release of precipitation frequently triggers off the growth of a new cell downwind. This may be brought about by a layer of cold air, about 1 km in depth, spreading out from the downdraught at ground level and causing lifting of unstable air downwind.

Cumulonimbus cells have an area of the order of a few square kilometres, often only a fraction of the total base area of the cloud. It must not be assumed that conditions are uniform over the whole horizontal extent of a cell which may include several regions of particularly strong updraughts (*Thunderstorm* 1949, Kuettner 1950). Cloud base is usually at a height of 1 to 3 km, while the height of the cloud top varies widely. In a typical moderate shower, the depth of the cloud might be 3-5 km, but in a violent thunderstorm, the cloud may be 15 km deep and may reach the tropopause (*Thunderstorm* 1949). Bowen (1950) and Ludlam (1951a, 1952) suggest that convective clouds cannot rain unless their depth exceeds about 2 km and this is well supported by observation.

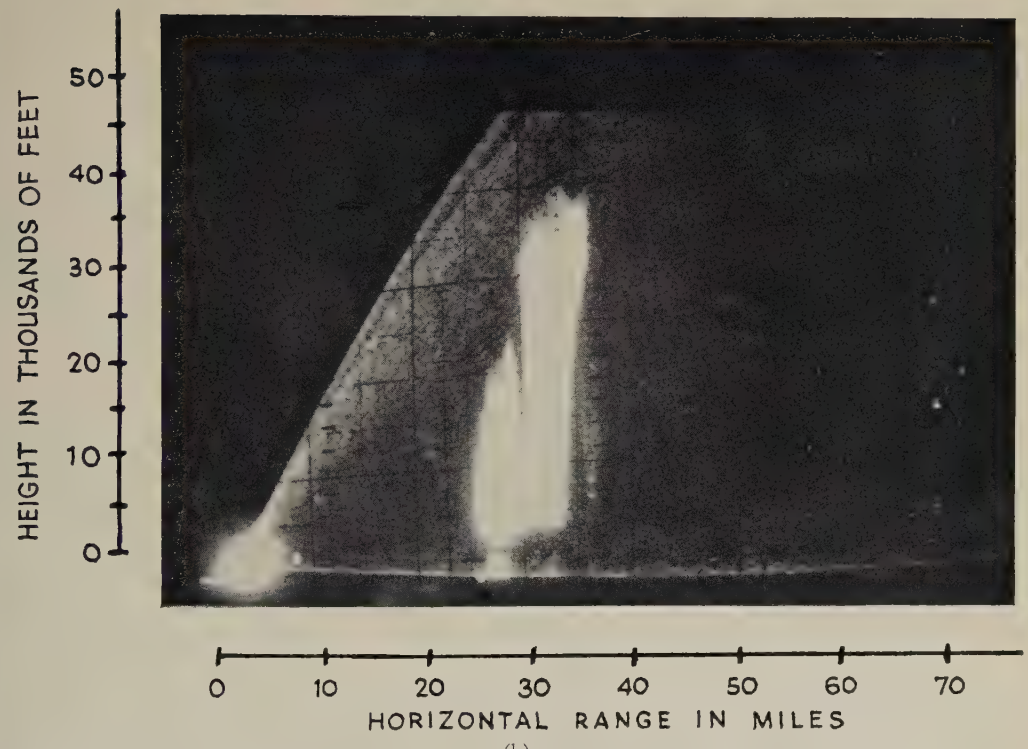
4.22 *Updraughts and turbulence*

The mean updraught in a cumulonimbus cell varies from 1 m/sec (small cloud) to perhaps 10 m/sec (thunderstorm). As might be expected from the equation of continuity, the updraughts increase upwards, and the greatest are found at about the 5-km level (*Thunderstorm* 1949). Updraughts have been measured by aircraft and by observing the rate of rise of the radar-echoing volume in a cloud, the two values being in excellent agreement. Higher values of updraught in cumulonimbus have been occasionally reported, Gillmer and Nietsch (1944) quote a report by a glider pilot of 100 m/sec, while Bilham and Relf (1937) have deduced velocities of 130 m/sec, on the assumption that the updraught must be sufficient to support the hailstones which sometimes fall from these clouds (see § 3.11). However, certain calculations (Gaviola and Fuertes 1947, Ludlam 1951a) suggest that hailstones can grow to the observed size without being supported by updraughts. Sil (1950) has estimated updraughts of up to 6 m/sec from the rainfall rate, on the hazardous assumptions that the cloud is in a steady state and that there is no entrainment (see below). Downdraughts in cumulonimbus cells occur, over regions about 1 km in diameter, and with modal value of 5 m/sec. The greatest speeds are found near the ground (*Thunderstorm* 1949).

Considerable effort has been put into measuring the vertical components of turbulent gusts in cumulonimbus (Jones 1949, 1954; *Thunderstorm* 1949). As might be expected, the greatest gusts are found in the regions of large vertical shear on the edges of cells. The value of the gust speed in terms of the equivalent sharp-edged gust (see e.g., Eyraud 1949) is obtained from vertical accelerations experienced by aircraft, and is commonly in the range 0 to 10 m/sec. Little is known of the spectrum of turbulence in convective cloud.



(a)



(b)

Figure 8. Radar echoes from cumulonimbus clouds showing cell structure. Fig. 8a is a fine example of an echo from a single isolated cell. Fig. 8b shows a pair of cells, of which that on the left has just reached the mature stage and is developing vigorously at the edge of an older cell. The tapering appearance of the latter may be due to attenuation of the radio waves by liquid water, which might be expected to show an increased concentration towards the base of the cells (cf. § 6.1). It should be noted that the scales of the horizontal and vertical axes are unequal, with a ratio of about 5 : 1, so that, in fact, the echoing regions have almost equal height and breadth.

The radar operated at a wavelength of 10 cm with a pulse-length of 2 μsec, and used an aerial scanning in a vertical plane and giving a beam-width measured between half-power points of 15° in azimuth and 3° in elevation. The photographs were obtained by R. F. Jones at East Hill, Dunstable, and are reproduced by permission of the Director of the Meteorological Office.

4.23 *Composition of convective clouds*

All types of precipitation elements may be found in convective cloud – rain, hail, graupel, snow and ice crystals. There has been little systematic observation of their occurrence in different regions of cloud at various stages in its development. The Thunderstorm Project (1949) found snow above 5 km in over 70 per cent of the observations in Florida and Ohio; its frequency increased with height. Hail seems to be rare and was encountered in less than 5 per cent of the observations, being found most often between 3 and 5 km above sea level; it is probably even more rare in convective shower clouds. It is suggested (*Thunderstorm* 1949) that hail is found, if at all, mostly during the mature stage; the evidence for this is very slender. Observers at the ground, on the other hand, report that hail, especially in the form of graupel (soft hail or snow pellets), is very common in precipitation from cumulonimbus clouds. Prohaska (1900) analyzed observations by an extensive network of observers in Austria, and showed that hail is often distributed by a storm over a track 1-2 km in width, and many kilometres long. His findings are supported by some valuable observations made at a mountain observatory on the Zugspitze (10,000 ft) by Kuettner (1950), who concludes that graupel occurs in 75 per cent of storms. The apparent discrepancy between reports from observers on the ground and from those in aircraft might be explained by the break-up of pellets of graupel which strike the windscreen of an aircraft, so giving the appearance of snow rather than of hail. For instance, one of the present authors noticed during a shower that pellets of graupel hitting the windscreen of a car travelling at a speed of 25 mi/hr (40 km/hr) remained intact, but at greater speeds they broke on impact.

The presence of liquid water, indicated by aircraft icing, was reported in roughly one-third of the observations at 9 km, the greatest height for which reports are given.

Ice crystals become a noticeable feature in the dissipating stage, when they predominate above 8 km (*Thunderstorm* 1949) forming the familiar anvil,* but their presence in earlier stages of the cell's life is a matter for conjecture. Thus little can be deduced from these observations concerning the importance of ice in initiating the release of rain.

4.24 *Liquid-water content and drop-size distribution*

Liquid-water contents in large cumulonimbus have usually been measured by ' rate-of-icing ' meters and such methods can give serious underestimates if the water content is so high that the rapid release of latent heat of fusion prevents some of the deposited water from freezing (Ludlam 1951b). Warner and Newnham (1952) have made determinations in small cumulus by measuring the change in electrical conductivity of absorbent paper exposed to the cloud, while others have used impactor techniques (Diem 1948, Squires and Gillespie 1952, Weickmann and Aufm Kampe 1953). The results may be briefly summarized as follows : in large thunderstorms, the liquid-water content reaches a maximum at a level about 2 km above cloud base (a result not predicted by simple theories) with a value of about 4-6 g/m^3. In small shower clouds, the water content is 0·2-0·5 g/m^3, and is usually less than would be expected from calculations on the basis of adiabatic ascent through the cloud base. This is almost certainly due to what has been called ' entrainment,' the mixing of the environment with ascending cloud air (Stommel 1947). There is no doubt that entrainment occurs, though there is disagreement on its mechanism (Malkus; also Scorer and Ludlam 1953). Even in large cumulonimbus, it can produce as much as 100 per cent dilution of the cloud air for every 400 mb of ascent

* It is not certain that the fibrous appearance of the anvil is an entirely reliable sign of glaciation. One of the authors recalls an occasion when the moon, visible through anvil cloud blowing off from distant cumulonimbus, was surrounded by a water diffraction corona.

(*Thunderstorm* 1949). On occasions, however, and particularly in large cumulonimbus clouds, the liquid-water content is found to be considerably greater than that expected from a simple calculation. Braham (1952) and Malkus (1953) have suggested that this can occur when a cell grows in an environment made moister than the average by the dissipation of previous cells, so that one cell feeds on the remains of its ancestors.

The mean droplet concentration (i.e. number per unit volume) in small cumulus is greater than that in larger clouds (cumulus congestus, cumulonimbus) which show, on the other hand, a higher proportion of droplets exceeding 50 μ in diameter (Weickmann and Aufm Kampe 1953). This may provide evidence for the growth of cloud droplets by coalescence, a point which will be discussed more fully in Section 5. However, droplets of diameter about 300 μ have often been detected in appreciable concentrations even in cumulus humilis (Ludlam, Browne and Day 1954). Their presence is very surprising, in view of the short lifetime, about 20 min, of these clouds.

5. Cloud budgets

5.1 *Layer clouds*

Measurements of the radar echo from precipitating clouds may be used to supplement the information given by the rainfall rate and by measurements of the raindrop-size distribution at the ground. If all such information is used together, there is sufficient to justify numerical estimates of mass, concentration (number per unit volume) and rate of growth of the precipitating particles. The case of rain-forming layer cloud is attractive,

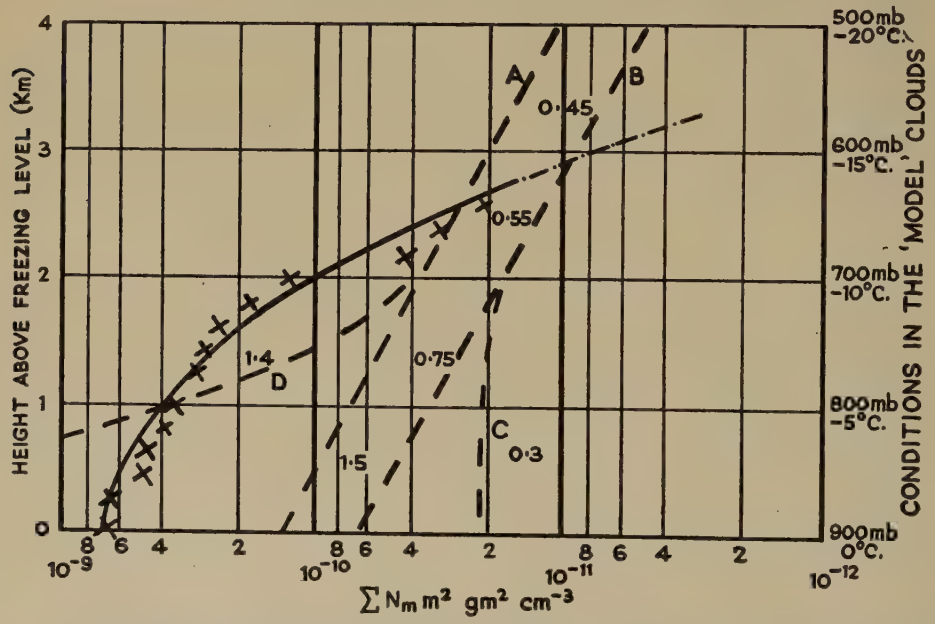


Figure 9. Graphs of $\sum_m N_m m^3$ against height above freezing level. The points × show the means of the observed values for three cases of widespread steady rain giving about 1 mm/hr at the ground. The small-chain line shows the trend of the echoes as they become too weak to observe. The broken lines show the values of $\sum_m N_m m^3$ calculated for model clouds giving a rainfall of 1 mm/hr. A. Concentration of particles 2/litre; B. Concentration of particles 1/litre; C. Particles of mass greater than 100 μg release splinters of mass 10 μg at the rate of one for every 500 m fall; D. Particle concentration reduced by aggregation, by a factor of 2 for every 500 m fall below the − 10°C isotherm. The figures on the graph give the calculated relative humidity in an equilibrium cloud, expressed as a fraction of the difference in vapour pressure between water and ice at the prevailing temperature. For this calculation the crystals are assumed to be circular discs, with thickness equal to one-fifth of the radius.

because conditions are reasonably uniform over a wide area, and as a tentative preliminary we may assume, with Bannon (1948), that the cloud is in a steady state. In other words, it can be assumed that all the water condensed from the updraught at any level is removed at the same rate by the growth of the precipitation particles falling through that level. The water budget of the cloud is balanced.

5.11 *The radar echo*

The intensity of the radar echo from a cloud is proportional to the quantity $\Sigma N_m m^2$, where N_m is the concentration of particles of mass m each with a shape factor of unity (see Section 6). Measurements of the echo intensity from different heights in a cloud, together with absolute measurements of the power and sensitivity of the radar set, enabled Browne (1952a) to investigate the magnitude and variation with height in layer cloud of the quantity $\Sigma N_m m^2$. Fig. 9 shows the measurements of $\Sigma N_m m^2$ in the cloud above the freezing level, for three cases of widespread warm-frontal rain, with precipitation rates of 0·9, 1·1 and 1·2 mm/hr, and freezing levels at 2,200 m, 1,180 m, and 960 m respectively.

In principle one could, by the following method, obtain information from these measurements about the way in which the precipitation particles in the cloud are growing. Starting from measurements of the rainfall rate and drop-size distribution N_m at the ground, a step-by-step calculation will yield values of N_m and m at any height in the cloud, for a given set of assumptions about the processes of growth and about the conditions in the cloud. The values so obtained could then be compared with the quantity $\Sigma N_m m^2$ obtained from the radar measurements as a test of the validity of the assumptions. The necessary combination of measurements is not yet available for an individual cloud, but it is instructive to compare the values of $\Sigma N_m m^2$ calculated for a mathematically simple model cloud with the values measured by Browne. Wexler (1952) and Browne (1952a) have both considered such simple models, and have attempted to relate them to Browne's echoes, although Wexler did not attempt to obtain quantitative agreement between the calculated and measured values of $\Sigma N_m m^2$. Their conclusions were very different, though neither made the assumption of a steady state.

5.12 *A model cloud*

The simplest model is a cloud in a steady state in which ice crystals are growing by sublimation. The rainfall rate is taken to be 1 mm/hr and the wet-bulb potential temperature of the air rising through the cloud base is taken as 5°C. Bannon's calculations (1948) show that under these conditions, the updraught in the cloud will be 5·5 cm/sec, and that water will be condensed from it at the rate I shown in the table :

Temperature (°C)	+ 2·5	− 2·5	− 7·5	− 12·5	− 17·5
Pressure (mb)	950	850	750	650	550
$I \times 10^{-11}$ (g cm^{-3} sec^{-1})	9·5	7·5	5·8	4·3	3·0

This table will be used to calculate the increase in mass of particles as they fall through the cloud, given their concentration and terminal velocity, without any assumptions as to their manner of growth.

Best (1950b) has summarized the available information about raindrop-size distributions. In an earlier paper (1947) he gives an expression for the mean mass of raindrops $\overline{m}$ (μg), which can be re-written in the form :

$$\overline{m} = 180\, p^{0.75}$$

where p is the rainfall rate in mm/hr. Thus for the model cloud under consideration,

the mean raindrop mass at the ground is 180 μg. Since the radar evidence suggests that the mass of the drops changes by less than 10 per cent as they fall from the freezing level to the ground, the ice crystals in the model will be assumed to be homogeneous with a mass, just above the freezing level, of 180 μg. Their terminal velocity will be taken as constant, with a value of 70 cm/sec (cf. § 3.11) so that the crystal concentration is 2/litre. The step-by-step calculation of the mass of the crystals at higher levels gives the values of $\Sigma N_m m^2$ plotted in Fig. 9, curve A. Curve B shows the values of $\Sigma N_m m^2$ for a similar cloud giving raindrops twice as massive and with half the concentration.

If the ice crystals are formed near the top, or coldest part of the cloud, their concentration may be increased as they fall by the release of ice splinters. This would retard the growth of the precipitating particles, and curve C shows the reduced values of $\Sigma N_m m^2$ that would be expected if splinters of mass 10μg were released at the rate of one per 500 m of fall by all crystals of mass greater than 100μg. The reverse effect can occur where crystals aggregate to form snowflakes, and curve D shows the marked increase in $\Sigma N_m m^2$ to be expected when aggregation reduces the concentration of particles by a factor of two for every 500 m fallen below the $-$ 10°C isotherm. Variants of the cloud model which include processes such as aggregation and splintering will, of course, give a rainfall which does not agree with the relation given by Best.

A more realistic investigation would take account of the unequal sizes of the ice crystals. Most measurements of the size distribution in rain show that the mean square mass $\overline{(m)^2}$ of the particles exceeds the square of the mean mass $(\overline{m})^2$ by a factor which can be as great as ten. The calculated values of $\Sigma N_m m^2$ for a homogeneous cloud will therefore be less than those for a natural cloud giving the same precipitation rate. This might remove the discrepancy shown in Fig. 9 between the calculated and measured values of $\Sigma N_m m^2$ at the freezing level, but seems unlikely to make the form of the calculated curve fit the observations.

5.13 *The humidity in ice clouds*

The assumption of a steady state has given the rate of growth of ice crystals. The relative humidities needed to achieve this rate can be calculated from the equations in § 3.24, by making suitable assumptions about the shape of the crystals. The figures on the curves in Fig. 9 give values of the supersaturation computed in this manner, and expressed as a fraction of the vapour-pressure difference between ice and water surfaces at the relevant temperatures. The crystals were assumed to be circular discs, of thickness equal to one-fifth of their radius. The calculated humidities are sensitive to this assumption about the crystal thickness : the thinner the crystals, the larger their radius, and the lower the supersaturation needed for a given rate of growth. The principal difference between the model clouds of Browne and Wexler lay precisely in the thickness assumed for their crystals. Browne was led to conclude that his crystals would not grow fast enough even at water saturation unless accretion was postulated, while Wexler found that the ice crystals must splinter in order to give a radar echo resembling that observed.

In the case of a pure ice cloud the supersaturation can change to maintain the equilibrium rate of growth of the ice crystals. For example, should the crystal concentration fall, equilibrium could be maintained by a rise in the relative humidity. But if liquid droplets are present in the cloud, the relative humidity is held close to water saturation, so that equilibrium can no longer be maintained by variations in humidity. It may thus be unrealistic to speak of a true steady state in a precipitating water cloud. Measurements are needed to establish whether or not warm-frontal nimbostratus contains supercooled water droplets above the freezing level.

5.14 *Discussion*

The estimates of relative humidity indicate a difficulty. If atmospheric ice crystals are formed only when the relative humidity rises to water saturation, as is found in the laboratory, they could not be formed at the top of our equilibrium cloud, where the relative humidity only slightly exceeds ice saturation (Fig. 9). If the crystals become very scarce at the top of the cloud, the relative humidity may rise to water saturation. Freezing nuclei would then be enabled to form ice crystals there, but this would cause the relative humidity to fall again. Thus the cloud would no longer be in true equilibrium, though the size of the irregularities that would occur is uncertain, and might be small enough (with linear dimensions of the order of ten metres) to give an apparently uniform radar echo and precipitation pattern at the ground.

Marshall (1953) has reported radar observations of a rather different type of irregularity. He has observed bands of enhanced echo intensity from the upper parts of nimbostratus, well above the freezing level, and has interpreted them, in agreement with Browne (1952b), as the echoes from precipitation streaks. The streaks appear to retain their identity in some cases for hours, and it is suggested that they arise from ' generating elements ' at the top of the cloud, which must in this case provide a steady supply of ice particles with similar falling speeds. The nature of the processes leading to the formation and subsequent maintenance of precipitation streaks is intriguing, and calls for further investigation.

The model cloud represented by curve A in Fig. 9 contained a concentration of ice crystals of 2/litre. If these crystals were formed by freezing nuclei carried in the air rising through the cloud base, the necessary concentration of freezing nuclei would be greater in the ratio of the terminal velocity of the ice crystals to the velocity of the updraught. The resulting value of 30 freezing nuclei/litre was estimated by Palmer (1949), while Aufm Kampe and Weickmann (1951) report similar estimates by an independent method. The source of the ice crystals is uncertain, but if they are formed by the freezing nuclei detected in the measurements shown in Fig. 6, the required concentration will not appear until the temperature reaches $-20°C$ to $-25°C$. However, rain, which has probably been formed by the Bergeron process, frequently falls from layer clouds with summit temperatures above $-15°C$. Thus there appears to be a shortage of freezing nuclei, and further investigations seem called for.

Brewer and Palmer (1949) have suggested that the additional ice crystals may originate as small splinters of ice released from a growing crystal. A few freezing nuclei might thus provide a much larger number of ice crystals, and might even initiate a self-sustaining chain-reaction.

To conclude this section, it is suggested that widespread steady rain from a layer cloud in true equilibrium is rare, and that small-scale equilibrium is impossible if the cloud contains both liquid water and ice above the freezing level. The precipitating ice crystals would then grow by accretion as well as by sublimation, but there could be no automatic control of the equilibrium as in an ice cloud. Calculations are difficult for this case, partly because the collection efficiencies of ice crystals are unknown.

5.2 Convective rainclouds

5.21 *Macrophysical problems*

The meteorologist would be satisfied with his knowledge of convective cloud if it enabled him to forecast the amount and rate of rainfall on a given day merely from detailed local air soundings and knowledge of the synoptic situation. There are wide gaps in his understanding which prevent him from achieving this ideal.

At present the heights of base and summit of clouds, less than about 3 km deep, can be predicted with an accuracy of perhaps $\pm$ 200 m from aerological soundings, provided that there is no large-scale vertical motion. There is a greater uncertainty in predictions of the heights of deeper clouds. Convergence increases convective activity and subsidence inhibits it, but their detailed effects have not been fully investigated.

The vigour with which a convective cloud builds up (that is, the updraught within the cloud) depends not only upon the instability of the air, but also upon the way in which hot air is fed in, often from sources on the ground (Scorer and Ludlam 1953), and the possibility of forecasting updraught velocities to better than $\pm$ 50 per cent seems remote. The reasons which determine whether the convection will manifest itself in the form of a few clouds of large base area, or in the form of many clouds of small base area, are imperfectly understood, but it is probably again determined largely by the distribution of heat sources. The problem is important, because the effects of entrainment upon the liquid-water content in the cloud are reduced as its base area is increased to provide the convective core with a protective shield of cloud. It has already been seen (§ 4.2) that aerological soundings are an unreliable guide to the liquid water content of cumuliform cloud.

In a paper based on the observations of the United States Thunderstorm Project, Braham (1952) discusses the history of the water vapour carried into a thunderstorm cell. He finds that, of some 10^9 kg of water vapour that may enter a single cell, only 50 per cent condenses : the remainder enriches the entrained air and passes through the top of the cloud. Of the condensed water, roughly two-fifths evaporate into the downdraught initiated by the release of precipitation, a further two-fifths are accounted for in the cloud remaining after the storm and by evaporation from the boundary of the cloud, and the rest (about $1{\cdot}2 \times 10^8$ kg) falls to the ground as rain. These estimates are very rough. As Braham points out, some thunderstorms in arid regions produce no precipitation at the ground; all the rain has been evaporated into the downdraught to maintain saturation during adiabatic descent. Braham estimates that the total energy release in a cell – the total of latent heats of evaporation and fusion – is of the order of 10^{22} erg. Over 95 per cent of this is ultimately used in altering the energy state of the environment, that is to say, in ' overturning ' the atmosphere, while most of the remaining energy is used in carrying liquid water aloft. Lightning discharges account for less than one per cent of the total energy.

It is clearly of importance to compile similar water and energy budgets for smaller convective showerclouds. Even if the resources of the Thunderstorm Project are not available, most of the necessary facts could be obtained by relatively simple experiments (see, e.g., Malkus 1953).

It has been seen that outstanding problems in the ' macrophysics ' of convective clouds are the prediction of the base area of clouds, the amount of entrainment, the updraught velocity and the liquid-water content. We may by-pass these problems, only to meet more; given the heights of cloud base and top, area of cloud base, central updraught velocity and the liquid-water content at all levels, can a detailed history of the subsequent rainfall be predicted ? These problems may be subdivided into those concerned with the initial formation of precipitation elements, and those concerned with the subsequent growth to raindrop size.

5.22 *The initiation of rainfall*

The two main processes put forward to account for the initial formation of rain in convective clouds have already been described in Section 1. They can be regarded as competing to form the first precipitation element which we may regard as a particle

of mass greater than 10 μg (e.g., a drop of diameter 0·27 mm). Once formed, a precipitation element will grow further by processes discussed in § 5·23 and, in so doing, will convert the growing into a decaying cloud.

The broad differences between the two initiation processes are that the Bergeron process requires the presence of a few ice particles among many supercooled water droplets, whereas the Bowen-Ludlam process requires a droplet population containing a few large members, together with a marked updraught and a sufficiently deep cloud. Most workers regard the giant sea-salt nuclei as the most likely source of these large droplets, accepting the implication that non-freezing rain is thus improbable in continental air masses. However, even in the absence of giant condensation nuclei, the initial droplet population formed at cloud base might conceivably alter in such a way that it contains many large droplets (of radius greater than 30 μ) by the time the air has risen a few hundred metres. Schumann (1940) has studied the effects of coalescence on droplet-size distributions, on the assumptions that collision frequency is independent of droplet size and that every collision results in coalescence. His calculations indicate that this process will create droplets sufficiently large to initiate the Bowen-Ludlam process only in a time considerably longer than 30 min, which may be taken as the lifetime of most droplets in cumuliform cloud. However, Schumann ignored the effects of turbulence in promoting collisions, effects which may be very important, as shown in a recent paper by East and Marshall (1954). For example, to increase the number of collisions between cloud droplets by factors from 5 to 10, the turbulent velocity spectrum must be such that the Fourier components of velocity in the frequency range 100-200 c/s have an amplitude of the order of 2 cm/sec. Although little is known of the spectrum of turbulence in clouds, these parameters do not seem unreasonable. It would thus seem possible that turbulence can play an important role in changing the initial droplet population in such a way that rain formation by the Bowen-Ludlam process becomes feasible, even in the absence of giant hygroscopic condensation nuclei.

It is often assumed that the presence of ice in showerclouds provides unambiguous evidence for the Bergeron process. However, the work of Bigg (1953b) has shown that the probability of a drop freezing under given conditions may be approximately proportional to its volume, so that a cloud containing large supercooled raindrops is very much more likely to contain ice than one which contains only droplets. This may account for Bowen's observations (discussed in § 2.2) that clouds seeded with large water droplets sometimes produced hail as well as rain. It is probably safe to assume that the Bowen-Ludlam process accounts for the initial release of any rain from convective clouds, whether or not they contain ice, whose tops do not extend above the — 6°C isotherm. This is the highest temperature at which ice crystals have been reported in cumuliform cloud (Coons, Jones and Gunn 1949; quoted by Bigg 1953a).

Several workers have studied the role played by ice in the initial release of precipitation, from measurements of the height at which the first radar echo from a thunderstorm appears. A radar echo from a cloud usually indicates the presence of elements larger than 10 μg in considerable numbers, so that its first appearance can be taken as a sign that the release of precipitation has begun. Workman and Reynolds (1949b) and the Thunderstorm Project (1949) found that the initial radar echo nearly always appears near the — 10°C isotherm; Battan (1953) on the other hand, concludes from an analysis of Thunderstorm Project data for Ohio that the initial echo frequently appears below the freezing level. Wexler (1953) has criticized Battan's work on the grounds that, under certain conditions, echoes from ice may be too weak to detect. Even if Battan's analysis is rejected, it is difficult to accept the conclusion that initial echoes near the — 10°C isotherm necessarily imply the presence of ice. For example, in a cloud of liquid-water

content 1 g/m^3 and updraught 3 m/sec, droplets growing by coalescence would give an appreciable radar echo only after they had been carried some 3 km above cloud base, a level which might well lie near the -10°C isotherm. It can be seen that observations of the initial radar echo alone cannot lead to unambiguous conclusions. They would be more valuable if correlated with simultaneous observations of the actual updraught (measured directly) and the liquid-water content.

5.23 *The growth of precipitation elements*

The general expression for the rate of growth of a precipitation element by both accretion and condensation is given by (cf. Section 3),

$$\frac{dm}{dt} \doteq \underset{\text{collisions}}{\pi r^2 v w E} + \underset{\text{condensation}}{4\pi \Delta f C M p/(\mathbf{R}T)}$$

where r = effective radius (cm) of element of mass m (g).

v = terminal velocity in cm/sec.

w = liquid-water content in g/cm^3.

E = mean collection efficiency.

Δ = diffusion coefficient (H_2O in air) in cm^2 sec^{-1}.

f = Frössling's wind factor (cf. Section 3).

p = vapour-pressure difference over the element (dyne/cm^2).

C = constant numerically equal to electrostatic capacity of the element (cf. Section 3) in cm.

Although this expression is generally difficult to integrate, a great simplification is usually possible in convective showerclouds, where the rate of growth by collision is far greater than that by condensation. For this condition to hold, it can easily be shown (putting $\Delta = 0{\cdot}2$ cm^2 sec^{-1}, $M/\mathbf{R}T = 8 \times 10^{-10}$ cm^{-2} sec^2, $p = 200$ dyne/cm^2, $E = 0{\cdot}3$, $v = 50$ cm sec^{-1}) that the radius of the element must exceed a limit given approximately by

$$r > 10^{-8}\, w^{-1} \text{ cm}$$

Thus for a cloud of liquid-water content 1 g/m^3, the growth of a precipitation element of radius greater than 100 μ will be almost entirely due to accretion. We may thus reasonably assume that the element is nearly spherical when it is big enough to be detectable by radar. Under these conditions, the rate of increase in the radius of the element with respect to distance h, fallen *relative to the ground*, is given by

$$\frac{dr}{dh} = \frac{w E(r)}{4\rho (1 - W/v)}$$

where ρ is the density of the element assumed homogeneous, W = updraught in the cloud. In the case of a water drop ($\rho = 1$ g/cm^3) this expression can be integrated fairly easily, as has been done by Bowen (1950) and Ludlam (1951a) (cf. Section 1). When dealing with ice elements, there is uncertainty in the value of ρ, which may also vary from the centre to the outside of the element. For example, when the rate of accretion is small (w and r small) the accreted water freezes immediately to form a soft rime (graupel) with many air inclusions, so that the density of the element may fall as low as 0·3 g/cm^3. As

the rate of accretion rises, the element forms a pellet of opaque hail, with somewhat increased density. Ludlam (1952) has shown that for high rates of accretion (large w, large r), the latent heat released by the freezing of the accreted water may be sufficient to raise the surface of the hailstone to 0°C. The water will now freeze only slowly, producing clear ice. Ludlam has suggested that the onion-like structure of some hailstones – alternate layers of clear and opaque ice – has been produced by their passage through regions of cloud with alternately high and low liquid-water content, clear ice being formed where the liquid-water content is so high that the accreted water does not freeze instantaneously, and opaque ice where the rate of accretion is low. Coste (1940) has pointed out, however, that opaque rime does not necessarily imply the presence of air inclusions. Ice may also be rendered opaque by dissolved gases coming out of solution on freezing (Leduc 1906).

Ludlam also considers that when the rate of accretion is so high that the hailstone acquires a coating of water, the water will blow off into the wake before freezing, and so provide further precipitation elements. The thickness and diameter of the water layer necessary for this to happen are not known (though they might be found out by wind-tunnel experiments), so that the suggestion must be regarded as speculative.

Langmuir (1948) pointed out that if a drop growing by coalescence reaches a diameter of 5 mm it will disrupt into a few large and many small fragments (§ 3.14). The large fragments would continue falling as rain, but the small fragments could be carried to the top of the cloud in an updraught and so recommence the process, which would thus become a self-propagating chain-reaction. The importance of this process has probably been overestimated; the conditions necessary for it to happen are so critical that they will be realized very seldom. Browne 1952a (see also Wormell 1953) has found at the core of large thunderclouds a typical radar echo with amplitude increasing exponentially with decreasing height, suggesting the occurrence of a similar type of chain-reaction which is, however, not self-propagating, but sustained by a supply of small raindrops from above. He has shown that, in order to fit both the observed rainfall rate and the absolute echo intensity, it is necessary to assume that the drops break when their diameter exceeds 3 mm. As shown in Section 3 this would require the presence of frequent gusts of the order of 10 m/sec in the cloud, a requirement which has only rarely been met (§ 4.22). However, this type of radar echo could also be explained by a straightforward coalescence process in the presence of an updraught of velocity greater than 1 m/sec.

6. Use of radar in cloud studies

Although radar observations may be made to yield various types of information relevant to the physics of clouds, the applications of this technique have been hitherto largely qualitative. Radar has been used for 'watching' clouds rather than for probing them. Since the literature on the subject is contained largely in non-meteorological journals a brief review is timely. The pictorial uses of radar are well known and will not be discussed here (see, e.g., Jones 1950). In many ways radar has advantages over aircraft for the experimenter – the initial cost is less, running costs are negligible, the reliability is greater and, lastly, no disturbance is caused to a cloud when a radar beam is directed into it. It can easily be shown that the heating due to absorption of the incident energy is negligible. The disadvantages of radar are that it cannot, unless airborne, cover a large area in a search for clouds, and that it can be used to investigate only a few parameters of the cloud, namely, its motion and extent, the presence or absence of ice, the mean square mass of water or ice present per unit volume, the shape of the particles and their velocity spectrum. This last quantity can lead to a direct measure of turbulence and updraught.

6.1 *The physics of scattering by meteorological particles*

The theory of the scattering of radio waves by meteorological particles, of diameter less than one-tenth of the wavelength, predicts that the echo intensity (power received) is proportional to the quantity $\Sigma N_m m^2$, where N_m is the number of particles of mass m per unit volume of cloud (Ryde 1946). The full equation expressing this may be written

$$I = \frac{C' \sum_m [N_m m^2 s_m(\kappa) f_m(\kappa)] \exp\left(-\int_0^R \gamma(R)\, dR\right)}{\lambda^4 R^2}$$

where I = power received,

C' = a constant involving several parameters of the radar set,

λ = the radar wavelength,

and R = the range from which the echo is received,

κ = is the dielectric constant of the scattering particles,

$s(\kappa)$ = a shape factor, equal to unity for spherical particles,

$f(\kappa)$ = a scattering factor, approximately equal to $(\kappa - 1)^2/(\kappa + 2)^2$,

m = is the mass of a particle,

N_m = the number of particles, of mass m, per unit volume,

and $\gamma(R)$ = the 'two-way' absorption coefficient.

This assumes that the cloud completely fills the radar beam. If it does not, as may happen at ranges greater than 10 km, the exponent of R is changed from 2 to 4.

For wavelengths between 1 and 10 cm, the values of $f(\kappa)$ for water and ice are respectively 0·92 and 0·17, so that, other things things being equal, a water cloud returns an echo five times more intense than that returned by a similar ice cloud.

Marshall, Langille and Palmer (1947) were the first to investigate Ryde's equation experimentally and were able to confirm the proportionality between I and $\Sigma N_m m^2$ for precipitation rates up to 10 mm/hr. Hooper and Kippax (1950b) verified the equation absolutely using radars operating simultaneously on wavelengths of 1·3, 3 and 9 cm, during steady rain, giving precipitation rates up to 2 mm/hr. Their work was extended by Browne (1952a) to showers giving precipitation rates up to 6 mm/hr and to snow. In all these measurements for rain and for snow, the predicted and measured values of echo intensity agreed to within the limits of experimental error, about $\pm$ 30 per cent. Recently, attention has been turned to scattering by non-spherical particles. Labrum (1952a) and independently Browne and Robinson (1952) have shown theoretically that non-spherical particles depolarise incident plane-polarised radiation, and that the shape factor $s(\kappa)$ is approximately unity for all dry ice particles but may vary between 0·1 and 10 for melting ice particles or spheroids of water. These predictions were confirmed by Labrum (1952b) in a remarkable series of measurements on individual particles suspended in a waveguide, and by Browne and Robinson (1952) and Hunter (1954), who measured the cross-polarized component in the echo from clouds containing non-spherical particles. Scattering by inhomogeneous particles has been investigated by Aden and Kerker (1951) and by Labrum (1952a, b) who have shown that when only one-third of a sphere of ice has melted, the scattering cross-section has already risen to 0·91 of that obtaining when the sphere has wholly melted.

Attenuation of radio waves is produced by gases, water vapour, liquid water and ice and decreases rapidly with increasing wavelength. At wavelengths greater than 1·5 cm, only liquid water produces appreciable attenuation, but at shorter wavelengths water vapour and oxygen become important. Goldstein (1949) gives tables of the attenuation coefficient γ found under various conditions. Another form of attenuation, often more important than that due to the atmospheric constituents, arises in the thin film of rainwater or layer of snow which may be deposited on the radar scanner or perspex 'radome.' For example, with a rainfall rate of about 20 mm/hr, the film of water on a radome reduced the intensity of echo by 17 db (or 98 per cent) at a wavelength of 1·3 cm (Hooper 1949).

It can be seen that the theory of scattering and attenuation of radio waves, by cloud particles of any size, shape and composition, rests on a solid basis of experimental fact supported firmly by theory. This being so, it is remarkable that so little attempt has been made to exploit radar for a quantitative study of clouds. Some of the difficulties in the path of the experimenter are briefly surveyed in the remainder of this section.

It is impossible to use brightness-modulated displays for making accurate intensity measurements, for not only is the brightness of the area representing the echo a complicated function of echo intensity, but only a small range of intensities can be represented at any instant, the reason for this being that unless strong signals are 'limited,' they cause defocusing of the electron beam. Deflection-modulated displays permit accurate measurement of echo intensity at the cost of showing only a one-dimensional representation of the cloud at any instant. The ideal is to use both types of display simultaneously. Provided that great care is taken, in calibrating the radar, to determine the constant C' in Ryde's equation (see e.g., Hooper and Kippax 1950b), the quantity $\Sigma N_m m^2$ may be determined to within 30 per cent. If this error seems excessive in comparison with that usually expected from physical measurements, it should be remembered that adjacent regions of a cloud may return echoes differing in intensity by factors of more than a million.

After measurement, the quantity $\Sigma N_m m^2$ must be converted into some other more readily apprehended, such as drop mass m and concentration N_m. In an attempt to do this Browne (1952a) has measured the rainfall rate at the ground, this being equal to $\Sigma N_m mv$, where v is the terminal velocity of a drop of mass m, and from simultaneous radar measures of $\Sigma N_m m^2$, using a vertically directed beam, has found, by trial and error, a mean value of m at the cloud base (and hence a mean-cube value of drop diameter). From the variation of echo intensity with height, values of mean drop mass can then be deduced for all heights, given sufficiently steady precipitation. This method is open to criticism, in that the physical significance of mean particle mass at any height is obscure unless the particle-size distribution is also known. A surer approach would be to measure the drop-size distribution at the ground and then work upwards, but the size distribution may well vary considerably with height. In the next section a possible method of determining the drop-size distribution by radar will be discussed.

6.2 *Doppler shifts in radar cloud echoes*

Radio waves reflected from objects moving with a line-of-sight velocity v suffer a change in frequency, or Doppler shift, given by :

$$\delta f = 2v/\lambda$$

where λ is the radio wavelength. In a cloud a large number of reflecting objects are moving relative to one another besides possessing a mean line-of-sight velocity. The

mean velocity produces a frequency shift in the radar echo, while the relative motions produce fluctuations in the intensity of the echo. The fluctuations are conveniently described either by their power spectrum or by their auto-correlation function (see, e.g., Siegert (1943); Booker, Ratcliffe and Shinn 1950). Consider a radar with a vertically-directed beam of finite angular width. The cloud particles in the beam will possess a mean line-of-sight velocity by virtue of their fall under gravity and of any vertical draughts acting upon them. They will be moving relative to one another because they possess different terminal velocities, because of turbulence and because any horizontal motion due to wind will possess a small line-of-sight component which varies across the finite width of the beam; these effects will all contribute towards causing the echo to fluctuate in intensity. Finally, spurious fluctuations and frequency shifts in the echo will arise in the radar equipment itself – the formidable difficulties inherent in minimising these have been discussed elsewhere (Browne 1952a). When several factors are contributing simultaneously to the fluctuation in an echo, their effects may be separated by a simple theorem (Browne, to be published) which states that the observed temporal autocorrelation function of the echo is equal to the product of the autocorrelation functions of the fluctuations which would be produced by each factor acting separately. Thus, if echo fluctuations are being produced by differences in terminal velocities, by turbulence and by horizontal wind, the turbulence may be deduced from measurements of the fluctuations if both the velocity of the wind and the drop-size distributions are known.

Measurements of the fluctuations in radar echoes from clouds have been made by Hilst (1949) and by Browne (1952a). Hilst used a nearly horizontal beam and recorded the power spectrum of the fluctuations directly. He interpreted his results in terms of the variation of wind with height and of certain simplified drop-size distributions. Hilst's apparatus had the disadvantage that changes in the power spectrum occurring over periods less than about two minutes were not detected. Browne, on the other hand, used a vertical beam and measured the autocorrelation function of the echo by a straightforward but rather cumbersome method. He was able to show, under steady warm-frontal conditions, that the snow particles above the freezing level possessed a root-mean-square velocity relative to one another of 20 cm/sec, while the raindrops below the freezing level had a relative R.M.S. velocity of 120 cm/sec. Both these authors measured only relative motions among cloud particles. The measurement of the mean velocity of the particles presents greater difficulties but Barratt and Browne (1953) have described a simple if somewhat restricted method of overcoming them, and have reported a measurement of downdraught in a showercloud. The simultaneous measurements of the vertical velocities of drops and the total echo intensity from them can yield a determination of the drop-size distribution (Bartnoff and Atlas 1951).

6.3 *The melting band*

During the war, Canadian radar operators noticed that a layer near the freezing level in rainclouds returned an enhanced radar echo, to which they gave the name ' bright band.' Lately, this term has been replaced by ' melting band,' a convenient suggestion due to Bowen. The melting band provides many valuable clues to the cloud physicist (see Section 2) and a bref discussion of its origin is given here. Ryde (1946) gave the following explanation : suppose that snowflakes of unit reflectivity are falling towards the freezing level. As they melt, their reflectivity increases five times, but when melting is complete and the wet flakes have collapsed to form drops, their terminal velocity also increases by about five times, with, consequently, a five-fold decrease in particle concentration N_m. The echo intensity thus returns to its original value. Hooper and Kippax

(1950a) after an extensive series of measurements, found that Ryde's explanation was substantially correct, while Bowen (1951) has confirmed that the region of the cloud which returns the melting-band echo contains crystals of melting ice. Austin and Bemis (1950) and Browne (1952a) have modified Ryde's theory to include factors such as the aggregation of snowflakes near the melting region, vertical draughts, and variation of the radar pulse-length. A major modification to Ryde's theory has been necessitated by Labrum's recent work (1952a, b) on the shape factor.

Quantitatively, the intensities of the echoes from just above, within, and just below the melting region (I_i I_m and I_w respectively) can be written approximately :

$$I_m/I_i = s(\kappa_w)\,[f(\kappa_w)/f(\kappa_i)]\,(m_w/m_i)^2$$
$$I_m/I_w = s(\kappa_w)\,[(v_w - W)/(v_i - W)]$$

provided that the radar-pulse length is smaller than twice the depth of the melting region. The symbols have the meaning given in § 6.1 with the addition W = updraught velocity, while the suffixes i and w refer to the states of ice and liquid water respectively. A schematic diagram to explain how the various terms in the equations arise is shown in Fig. 10.

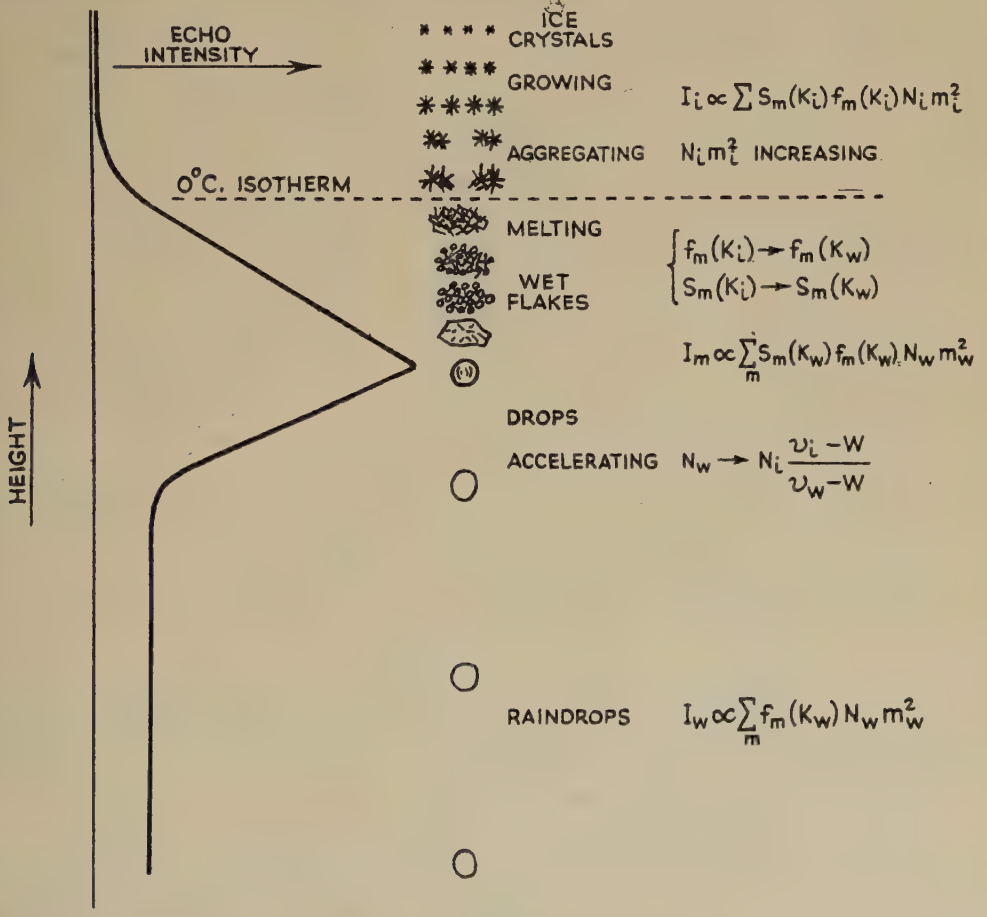


Figure 10. Schematic diagram to illustrate the formation of a ' melting band ' in the radar echo from a raincloud. On the left, the radar-echo intensity after correction for inverse-square variation with range is plotted against height. In the centre are symbolised the particles giving rise to the radar echo at various heights, together with an indication of their behaviour and physical state. The formal relations on the right express the effect on the echo intensity of the various processes occurring in the cloud, and should be compared with the appropriate equations in the main text, where the symbols are also defined.

7. Conclusion

Cloud physics has reached a stage at which the various processes leading to rain are understood qualitatively. One of the principal gaps in our knowledge concerns the actual conditions to be found inside rain-clouds – liquid-water contents, drop, crystal and droplet-size distributions, updraughts, the spectrum of turbulence (cf. § 5.22) and, in layer clouds, relative humidities above the freezing level. Although satisfactory methods for measuring some of these quantities, particularly liquid-water contents and droplet-size distributions, have been developed, there has been a tendency to postpone carrying out systematic measurements until the apparatus has been brought to a state of unnecessary perfection. The many neglected potentialities of radar have already been discussed in this connection in the previous section.

The influence of condensation and freezing nuclei upon global and temporal variations of rainfall offers an interesting field of study. Much valuable information could come from observations such as those of Smith and Heffernan (§ 3.23), but a more rapid yield of knowledge might be gained by a study of the fate of the individual artificial nuclei used in cloud-seeding experiments. It should not prove difficult to seed suitable cumulus clouds at base level with finely ground common salt (NaCl) which had been converted into the radio-active form containing the isotope Na^{24} by irradiation in a nuclear pile. It would be necessary to carry out the irradiation a few hours before the seeding experiment because the highly beta-active sodium-24 has a half-life of only 15 hr. Preliminary calculations indicate that rain falling from clouds which had been seeded with salt prepared in this way would be sufficiently radio-active to give an easily measurable disintegration rate per gramme, a careful measurement of which would indicate whether the rain had actually been initiated by the seeding, or whether the salt particles had merely been collected by rain which had formed naturally. The possibilities of seeding with radio-active silver iodide do not appear to be favourable, as none of the many artificial isotopes of silver or iodine are sufficiently radio-active for the purpose.

In his review of cloud physics in this *Journal* in 1941, Sir George Simpson concluded by asking some stimulating questions, which subsequent research has gone far to answer. We try to follow his example with some further questions :

(a) Is a steady-state process of rain formation possible, even in layer cloud ?
(b) Do ice crystals in nimbostratus usually grow in a water-saturated environment ?
(c) Do raindrops in the core of deep convective clouds grow as pellets of graupel, and are freezing nuclei concerned in the formation of the pellets ?
(d) Are the giant droplets necessary to the Bowen-Ludlam process likely to form naturally in continental air masses far inland ?

8. Acknowledgments

The authors are grateful to Dr. S. G. Frank and Dr. T. R. Kaiser for helpful discussions of certain matters reviewed in this article, and to Mrs. H. P. Palmer for checking the calculations in § 5.1.

References

Absalom, H. W. L. 1954 'Artificial production of precipitation,' *Air Ministry, Met. Res. Cttee., M.R.P.*, No. 834.

Adderley, E. E. 1953 ' The growth of raindrops in cloud,' *Quart. J. R. Met. Soc.*, **79**, p. 380.

Aden, A. L. and Kerker, M. 1951 ' The scattering of electromagnetic waves from two concentric spheres,' *J. Appl. Phys.*, **22**, p. 1242.

Aufm Kampe, H. J. and Weickmann, H. K. 1951 ' The effectiveness of natural and artificial aerosols as freezing nuclei,' *J. Met.*, **8**, p. 283.

Austin, P. M. and Bemis, A. C.	1950	'A quantitative study of the bright band in radar precipitation echoes,' *J. Met.*, **7**, p. 145.
Bannon, J. K.	1948	'The estimation of large-scale vertical currents from the rate of rainfall,' *Quart. J. R. Met. Soc.*, **74**, p. 57.
Barratt, P. and Browne, I. C.	1953	'A new method for measuring vertical air currents,' *Quart. J. R. Met. Soc.*, **79**, p. 550.
Bartnoff, S. and Atlas, D.	1951	'Microwave determination of particle-size distribution,' *J. Met.*, **8**, p. 130.
Battan, L. J.	1953	'Formation and spread of precipitation in convective clouds,' *Ibid.*, **10**, p. 311.
Bergeron, T.	1933	'On the physics of cloud and precipitation,' *Proc. Verb. Assoc. Mét. U.G.G.I.*, Lisbon, p. 156.
Best, A. C.	1947	'The size distribution of raindrops,' *Air Ministry, Met. Res. Cttee., M.R.P.*, No. 352.
	1950a	'Empirical formulae for the terminal velocity of water drops falling through the atmosphere,' *Quart. J. R. Met. Soc.*, **76**, p. 302.
	1950b	'The size distribution of raindrops,' *Ibid.*, **76**, p. 16.
Bigg, E. K.	1953a	'The formation of atmospheric ice crystals by the freezing of droplets,' *Ibid.*, **79**, p. 510.
	1953b	'The supercooling of water,' *Proc. Phys. Soc.* B, **66**, p. 688.
Bilham, E. G. and Relf, E. F.	1937	'The dynamics of large hailstones,' *Quart. J. R. Met. Soc.*, **63**, p. 149.
Blanchard, D. C.	1948	'Observations on the behaviour of water drops at terminal velocity in air,' *Project Cirrus, Occ. Rep., G.E.C.*, No. 7, Schenectady.
Booker, H. G., Ratcliffe, J. A. and Shinn, D. H.	1950	'Diffraction from an irregular screen with application to ionospheric problems,' *Phil. Trans. Roy. Soc.*, **242**, p. 579.
Bowen, E. G.	1950	'The formation of rain by coalescence,' *Austr. J. Sci. Res.*, A**3**, p. 193.
	1951	'Radar observations of rain and their relation to mechanisms of rain formation,' *J. Atmos. Terr. Phys.*, **1**, p. 125.
	1952	'A new method of stimulating convective cloud to produce rain and hail,' *Quart. J. R. Met. Soc.*, **78**, p. 37.
	1953	'The influence of meteoric dust on rainfall,' *Austr. J. Phys.*, **6**, p. 498.
Braham, R. R.	1952	'The water and energy budgets of a thunderstorm and their relation to thunderstorm development,' *J. Met.*, **9**, p. 227.
Brewer, A. W. and Palmer, H. P.	1949	'Condensation processes at low temperatures and the production of new sublimation nuclei by the splintering of ice,' *Nature*, **164**, p. 312.
	1951	'Freezing of supercooled water,' *Proc. Phys. Soc.* B, **64**, p. 765.
Browne, I. C.	1952a	Dissertation, Cambridge University.
	1952b	'Precipitation streaks as a cause of radar upper bands,' *Quart. J. R. Met. Soc.*, **78**, p. 590.
	1953	Contribution to a discussion, *Ibid.*, **79**, p. 442.
Browne, I. C. and Robinson, N. P.	1952	'Cross polarisation of the radar melting band,' *Nature*, **170**, p. 1078.
Browne, I. C., also Wexler, R. (*reply*)	1953	'The collection efficiency of ice crystals,' *Quart. J. R. Met. Soc.*, **79**, p. 549.
Byers, H.	1949	'Condensation nuclei and precipitation,' *J. Met.*, **6**, p. 363.
Byers, H. and Braham, R. R. (Eds.)	1949	*The Thunderstorm*, U.S. Weather Bureau, Washington, D.C.
Ceplecha, Z.	1953	'Meteor photographs. II. The photographed breakage of meteor 131a,' *Bull. Ast. Inst. Czech.*, **4**, p. 113.
Coste, J. H.	1940	'The trapping of air in the formation of rime,' *Quart. J. R. Met. Soc.*, **66**, p. 366.
Cunningham, R. M.	1951	'Some observations of natural precipitation processes,' *Bull. Amer. Met. Soc.*, **32**, p. 334.
Cwilong, B. M.	1947	'Sublimation in a Wilson chamber,' *Proc. Roy. Soc.* A, **190**, p. 137.
Dady, G.	1947	'Contribution à l'étude des précipitations,' *Comptes Rendus*, Paris, **225**, p. 1349.
Dessens, H.	1949	'The use of spiders' threads in the study of condensation nuclei,' *Quart. J. R. Met. Soc.*, **75**, p. 23.
Diem, M.	1948	'Messungen der Grosse von Wolkenelementen, II,' *Met. Rund.*, **1**, p. 261.

Dorsch, R. G. and Hacker, P. T.	1950	*N.A.C.A. Tech. Notes*, No. 2142.
East, T. W. R. and Marshall, J. S.	1954	'Turbulence in clouds as a factor in precipitation,' *Quart. J. R. Met. Soc.*, **80**, p. 26.
Eyraud, R.	1949	'Influence des courants verticaux sur les avions,' *J. Sci. Mét.*, **1**, p. 23.
Findeisen, W.	1938	'Die kolloidmeteorologischen Vorgänge bei der Niederschlagsbildung,' *Met. Z.*, **55**, p. 121.
	1939	'The problem of the formation of raindrops in clouds which are entirely composed of water,' *Ibid.*, **58**, p. 365.
Findeisen, W. and Findeisen, E.	1943	'Untersuchungen über die Eissplitterbildung an Reifschichten,' *Ibid.*, **60**, p. 145.
Findeisen, W. and Schulz, G.	1944	'Experimentelle Untersuchungen über die atmosphärische Eisteilchenbildung, I,' *Forsch. Erf. Reich. Wetterdienst.*, A, No. 27.
Frith, R.	1951	'The size of cloud particles in stratocumulus cloud,' *Quart. J. R. Met. Soc.*, **77**, p. 441.
Frössling, N.	1938	'Uber die Verdunstung fallender Tropfen,' *Gerl. Beitr. Geophys.*, **52**, p. 170.
Gabilly, A.	1949	'Sur le rôle que peuvent jouer les rafales dans la coalescence des gouttelettes d'un nuage,' *Ann. Géophys.*, **5**, p. 232.
Gaviola, E. and Fuertes, F. A.	1947	'Hail formation, vertical currents and the icing of aircraft,' *J. Met.*, **4**, p. 117.
Gillmer, T. and Nietsch, E.	1944	*Clouds, Weather and Flight*, New York (Van Nostrand), p. 117.
Goldstein, H.	1949	'Absorption and scattering of microwaves by the atmosphere,' from *Radio Wave Propagation*, New York (Academic Press), p. 269.
Gunn, K. L. S. and Hitschfeld, W.	1951	'A laboratory investigation of the coalescence between large and small water-drops,' *J. Met.*, **8**, p. 7.
Gunn, R. and Kinzer, G. D.	1949	'The terminal velocity of raindrops,' *J. Met.*, **6**, p. 243.
Heverley, J. R.	1949	'Supercooling and crystallisation,' *Trans. Amer. Geophys. Uni.*, **30**, p. 205.
Hewson, E. and Longley, R.	1944	*Meteorology, Theoretical and Applied*, New York (Wylie), p. 358.
Hilst, G. R.	1949	*Weather Radar Res. Tech. Rep.*, No. 9, A, M.I.T.
Hochschwender, E.	1919	Dissertation, Heidelberg University, transl. by W. R. Lane, Min. of Supply, C.D.R.E., Porton, 1949.
Hooper, J. E. N.	1949	'Measurements of radar echo intensities from precipitation,' *Min. Supply, Rep.*, No. T2116, T.R.E. Malvern.
Hooper, J. E. N. and Kippax, A. A.	1950a	'The bright band – a phenomenon associated with radar echoes from falling rain,' *Quart. J. R. Met. Soc.*, **76**, p. 125.
	1950b	'Radar echoes from meteorological precipitation,' *Proc. I.E.E.*, London, **97**, Pt. 1, p. 89.
Houghton, H. G.	1938	'Problems connected with the condensation and precipitation processes in the atmosphere,' *Bull. Amer. Met. Soc.*, **19**, p. 152.
	1950	'A preliminary quantitative analysis of precipitation mechanisms,' *J. Met.*, **7**, p. 363.
Hunter, I. M.	1954	'Polarisation of radar echoes from meteorological precipitation,' *Nature*, **173**, p. 165.
Jeffreys, H.	1918	'Some problems of evaporation,' *Phil. Mag.*, **35**, p. 270.
Jones, R. F.	1949	Contribution to a discussion on 'Turbulence in clear air and in cloud,' *Met. Mag.*, **78**, p. 114.
	1950	'Radar weather echoes,' *Ibid.*, **79**, pp. 109, 143, 170, 198.
	1951	'Rain from non-freezing clouds,' *Ibid.*, **80**, p. 273.
	1954	'Five flights through a thunderstorm belt,' *Quart. J. R. Met. Soc.*, **80**, p. 377.
Kaiser, T. R.	1953	'Radio-echo studies of meteor ionisation,' *Adv. in Phys.*, London, **2**, p. 495.
Kinzer, G. D. and Gunn, R.	1951	'The evaporation, temperature and thermal relaxation-time of freely falling water drops,' *J. Met.*, **8**, p. 71.
Kline, D. B. and Walker, J. A.	1951	*N.A.C.A. Tech. Notes*, No. 2306.
Kotsch, W. J.	1947	'An example of colloidal instability in clouds in tropical latitudes,' *Bull. Amer. Met. Soc.*, **28**, p. 87.
Krastanow, L.	1941	'Beitrag zur Theorie der Tropfen- und Kristallbildung in der Atmosphäre,' *Met. Z.*, **58**, p. 37.

Kraus, E. B. and Squires, P.	1947	'Experiments on the stimulation of clouds to produce rain,' *Nature*, **159**, p. 489.
Kuettner, J.	1950	'Electrical and meteorological conditions inside thunderclouds,' *J. Met.*, **7**, p. 322.
Labrum, N. R.	1952a	'The scattering of radio waves by meteorological particles,' *J. Appl. Phys.*, **23**, p. 1324.
	1952b	'Some experiments on centimetre wavelength scattering by small obstacles,' *Ibid.*, **23**, p. 1320.
Langleben, M. P.	1954	'The terminal velocity of snowflakes,' *Quart. J. R. Met. Soc.*, **80**, p. 174.
Langmuir, I.	1948	'The production of rain by a chain-reaction in cumulus clouds at temperatures above freezing,' *J. Met.*, **5**, p. 175.
Leduc, A.	1906	'Sur la densité de la glace,' *Comptes Rendus*, Paris, **142**, p. 149.
Lenard, P.	1904	'Uber Regen,' *Met. Z.*, **21**, p. 249.
Ludlam, F. H.	1951a	'The production of showers by the coalescence of cloud droplets,' *Quart. J. R. Met. Soc.*, **77**, p. 402.
	1951b	'The heat economy of a rimed cylinder,' *Ibid.*, **77**, p. 402.
	1952	'The production of showers by the growth of ice particles,' *Ibid.*, **78**, p. 543.
Ludlam, F. H., Browne, I. C. and Day, G.	1954	'Observations of small shower clouds,' (in publication).
Malkus, J. S.	1953	'Some results of a trade cumulus cloud investigation,' *Woods Hole Oceanogr. Inst. Tech. Rep.*, No. 23 (unpublished M.S.).
Malkus, J. S., Scorer, R. S. and Ludlam F. H. (*reply*)	1953	'Bubble theory of penetrative convection,' *Quart. J. R. Met. Soc.*, **79**, p. 288.
Marshall, J. S.	1953	'Precipitation trajectories and patterns,' *J. Met.*, **10**, p. 25.
Marshall, J. S., Langille, R. C. and Palmer, W. McK.	1947	'Measurement of rainfall by radar,' *J. Met.*, **4**, p. 186.
Mason, B. J.	1953	'The growth of ice crystals in a supercooled water cloud,' *Quart. J. R. Met. Soc.*, **79**, p. 104.
Mason, B. J. and Howorth, B. P.	1952	'Some characteristics of stratiform clouds over Northern Ireland in relation to their precipitation,' *Ibid.*, **78**, p. 226.
Mason, B. J. and Ludlam, F. H.	1950	'The microphysics of clouds,' *Rep. Progr. Phys., Phys. Soc.*, **14**, p. 147.
McDonald, J. E.	1953	'Erroneous cloud physics application of Raoult's Law,' *J. Met.*, **10**, p. 68.
Moore, D. J.	1952	'Measurements of condensation nuclei over the North Atlantic,' *Quart. J. R. Met. Soc.*, **78**, p. 596.
Mordy, W. A. and Eber, L. E.	1954	'Observations of rainfall from warm clouds,' *Ibid.*, **80**, p. 48.
Nakaya, U.	1951	'The formation of ice crystals,' *Compendium of Meteorology, Amer. Met. Soc.*, p. 207.
Palmer, H. P.	1949	'Natural ice particle nuclei,' *Quart. J. R. Met. Soc.*, **75**, p. 17.
	1950	Contribution to a discussion, *Cent. Proc. R. Met. Soc.*, p. 63.
Peppler, W.	1940	'Unterkühlte Wasserwolken und Eiswolken,' *Forsch. Erf. Reichs. Wetter.*, B, No .1.
Prohaska, K.	1900	'Die jährliche und tägliche Periode der Gewitter und Hagelfälle in Steiermark und Kaernten,' *Met. Z.*, **17**, p. 327.
Rayleigh, Lord	1879	'The influence of electricity on colliding water drops,' *Collected Papers*, **1**, p. 372.
	1882	'Further observations upon liquid jets,' *Ibid.*, **2**, p. 103.
Reynolds, S. E.	1952	'Ice crystal growth,' *J. Met.*, **9**, p. 36.
Ryde, J. W.	1946	'The attenuation and radar echoes produced at centimetre wavelength by various meteorological phenomena,' *Meteorological Factors in Radio-Wave Propagation, Phys. Soc.* and *R. Met. Soc.*, p. 169.
Schaefer, V. J.	1951	'Snow and experimental meteorology,' *Compendium of Meteorology, Amer. Met. Soc.*, p. 221.
Schumann, T. E. W.	1940	'Theoretical aspects of the size distribution of fog particles,' *Quart. J. R. Met. Soc.*, **66**, p. 195.
Scorer, R. S. and Ludlam, F. H.	1953	'Convection in the atmosphere,' *Quart. J. R. Met. Soc.*, **79**, p. 317.
Siegert, A. J. F.	1943	'The fluctuations in signal from many independently moving scatterers,' *Rad. Lab. Rep., M.I.T.*, No. 465.
Sil, J. M.	1950	'Relationship between vertical currents and intensity of precipitation,' *Indian J. Met., Geophys.*, **1**, p. 52.

Simpson, G. C.	1941	' On the formation of cloud and rain,' *Quart. J. R. Met. Soc.*, **67**, p. 99.
Smith, E. J.	1951a	' Observations of precipitation with an airborne radar,' *Austr. J. Sci. Res.* A, **3**, p. 214.
	1951b	' Observations of rain from non-freezing clouds,' *Quart. J. R. Met. Soc.*, **77**, p. 33.
Smith, E. J. and Heffernan, K. J.	1954	' Airborne measurements of the concentration of natural and artificial freezing nuclei,' *Ibid.*, **80**, p. 182.
Smith, L. G.	1951	Contribution to discussion, *Ibid.*, **77**, p. 683.
Squires, P. and Gillespie, C. A.	1952	'A cloud droplet sampler for use on aircraft,' *Ibid.*, **78**, p. 387.
Stickley, A. R.	1940	'An evaluation of the Bergeron-Findeisen precipitation theory,' *Month. Weath. Rev.*, **68**, p. 272.
Stommel, H.	1947	' Entrainment of air into a cumulus cloud,' *J. Met.*, **4**, p. 491.
Thunderstorm, The	1949	See Byers and Braham 1949.
Virgo, S.	1950	' Tropical rainfall from cloud which did not extend to the freezing level,' *Met. Mag.*, **79**, p. 237.
Warner, J. and Newnham, T. D.	1952	'A new method of measurement of cloud water content,' *Quart. J. R. Met. Soc.*, **78**, p. 46.
Wegener, A.	1910	' Uber die Eisphase der Wasserdampfs in der Atmosphäre,' *Met. Z.*, **27**, p. 453.
	1920	' Frost Ubersattigung und Cirren,' *Ibid.*, **37**, p. 8.
Weickmann, H. K.	1945	' Formen und Bildung der Atmosphärischen Eiskristallen,' *Beitr. Phys. frei. Atmos.*, **28**, p. 12.
	1953	' Observational data on the formation of precipitation in cumulonimbus cloud,' *Thunderstorm Electricity*, Univ. of Chicago Press, p. 67.
Weickmann, H. K. and Aufm Kampe, H. J.	1953	' Physical properties of cumulus clouds,' *J. Met.*, **10**, p. 204.
Wexler, R.	1952	' Precipitation growth in stratiform clouds,' *Quart. J. R. Met. Soc.*, **78**, p. 363.
	1953	' Radar echoes from a growing thunderstorm,' *J. Met.*, **10**, p. 285.
Wigand, A.	1919	' Die vertikale Verteilung der Kondensationskerne in der freien Atmosphäre,' *Ann. Phys.*, **59**, p. 689.
Woodcock, A. H.	1950	' Sea salt in a tropical storm,' *J. Met.*, **7**, p. 397.
Workman, E. J. and Reynolds, S. E.	1949a	*New Mexico Sch. Mines Prog. Rep.*, No. 6.
	1949b	' Electrical activity related to thunderstorm cell growth,' *Bull. Amer. Met. Soc.*, **30**, p. 142.
Wormell, T. W.	1953	'Atmospheric Electricity : Some recent trends and problems,' *Quart. J. R. Met. Soc.*, **79**, p. 3.
World Meteorological Organization	1954	'Artificial inducement of precipitation with special reference to the arid and semi-arid regions of the world,' *Tech. Note*, No. 1.
Wright, H. L.	1939	' Atmospheric opacity : a study of visibility observations in the British Isles,' *Quart. J. R. Met. Soc.*, **65**, p. 411.

Appendix

The desire to carry out a quick order-of-magnitude calculation is often frustrated by a lack of readily-found constants. Some rough constants are appended here, in the hope that they may be of use to those who, like the authors, are not so familiar with cloud physics as to have all the data at their finger-tips. The sources of most of the values are given in this review; the rest are standard.

1. *Terminal velocities* (cm/sec)

Ice crystals (diameter 100 μ – 1,000 μ)	70
Small snowflakes	100
Large snowflakes	150

Raindrops and droplets of diameter D

D	10 μ	50 μ	100 μ	500 μ	1 mm	3 mm	5 mm
v (cm/sec)	0·3	6	30	200	400	800	900

2. *Collection efficiencies E (D)*

For homogeneous clouds containing droplets of diameter (a) 12 μ, (b) 20 μ

	D	50 μ	100 μ	300 μ	1 mm	3 mm	5 mm
(a) 12 μ	$E(D)$	·05	·25	·5	·7	·7	·6
(*b*) 20 μ	$E(D)$	·4	·6	·7	·9	·9	·8

3. *Vapour pressure difference over ice in a water-saturated atmosphere*

The heating of the ice by latent heat has been taken into account

T (°C)	0	− 5	− 10	− 15	− 20	− 25
p (dyne/cm^2)	0	130	160	180	170	150

4. *Various constants*

Density of air (N.T.P.) $= 1{\cdot}3 \times 10^{-3}$ g cm^{-3}

Viscosity of air (N.T.P.) $= 1{\cdot}8 \times 10^{-4}$ g cm^{-1} sec^{-1}

Kinematic viscosity of air (N.T.P.) $= 0{\cdot}14$ cm^2 sec^{-1}

Diffusion coefficient of water vapour in air (N.T.P.) $= 0{\cdot}2$ cm^2 sec^{-1} $\propto$ (°K)^2

$M/\mathbf{R}T$ (water vapour) $= 8 \times 10^{-10}$ sec^2 cm^{-2} (see Section 3)

5. *Typical parameters for rainclouds*

Height and temperature of cloud base assumed to be $1\frac{1}{2}$ km and 5°C respectively

	Height of cloud top (km)	Updraught velocity (m/sec)	Liquid-water content (g/m^3)	Precipitation rate (mm/hr)
Layer cloud Ns	5	0·1	0·1	2
Small shower cloud (Cu congestus) (Cb)	4	2	0·5	10
Thundercloud Cb	8	10	2	80

551.509.3

The development of meteorology as an exact science

By O. G. SUTTON, C.B.E., D.Sc., F.R.S.

Presidential Address delivered before the Royal Meteorological Society on 28 April 1954

An 'exact science,' according to the dictionary, is 'one admitting of quantitative treatment,' as opposed to a purely descriptive summary of knowledge acquired by observation. In other words, an exact science is a branch of mathematical physics. It would be idle to pretend that meteorology, although it uses the language of physics and mathematics, has yet acquired this status in all its branches. To take an obvious example, routine weather forecasting from the synoptic chart as yet involves very little actual calculation and the reasoning with which the forecaster judges future developments is qualitative rather than quantitative, although it is essentially rooted in physics. My aim in this lecture is to attempt a critical examination of the part played by mathematics in meteorology – a stocktaking, if you will, to see how far we may apply the adjective ' exact ' to our science in the year 1954.

1. The mathematical method in science

Before we begin to examine the influence of mathematics on meteorology, it is as well to consider why mathematics enters into science at all. This is not a rhetorical question. The basis of pure mathematics, and therefore of all mathematics, is the concept of number, which in itself is a creation (but one of the most remarkable) of the human intellect. The introduction of number into logical reasoning has produced mathematics, which thus deals entirely with abstractions and has no essential connection with the world of concrete objects which we know through our senses. There is no *a priori* reason why we should expect rainfall, for example, to have anything in common with this system of thought, and to our not very remote ancestors (and even, perhaps, to some men of our own time) it would seem unreasonable and even impious to attempt to calculate in advance the amount of rain which falls on the growing crop.

However, we know now that there is a very remarkable relation between numbers and certain aspects of the inanimate universe as revealed by observation. Western science was founded on this truth. The relation was partly apprehended by those great mathematicians, the Greeks, and first fully realised by Galileo, Newton and their successors. From their studies we derive the modern view of the inanimate universe, and in particular our belief that rain, winds, storms and sunshine are not the work of benign or malevolent spirits, but simply manifestations, on an immense scale, of certain properties of matter, entirely similar to those which we can manipulate and observe in the quiet of the laboratory.

Physics begins with the recognition of certain fundamental observables, such as mass, length and time, with which the concept of number can be associated in an objective fashion. From this springs the idea of a *physical magnitude*. Other concepts, such as pressure, velocity, momentum and energy, are derived from the basic magnitudes by the mathematical operations of multiplication and division. A quantitative relation between derived magnitudes is called a *natural law*. Usually, this brings in a new number called a *physical constant*. Thus, to take a very familiar example, the pressure

(p), temperature (T) and density (ρ) of a gas in a state far removed from the risk of liquefaction are related by

$$p = R\rho T$$

where R is the gas constant. Modern science depends essentially on the fact that R has the same numerical value (in the same units) at all times and in all places. The framework of the science is completed by the choice of certain non-quantitative rules for selection of systems, such as the second law of thermodynamics, which cannot be verified by experiment but which allow the mathematician to select out of a large number of possible solutions those which are believed to be appropriate to the world revealed by our observations.

All this is, of course, very familiar and my reason for repeating these fundamental principles is because it is essential for my purpose that we recognize, from the outset, precisely what is involved in the belief that meteorology is a true physical science, that is, one which is amenable to this kind of discipline. Mathematics has two main uses in applied science. The first is to provide an objective method of summarizing experience – in fact, to act as a ' memory.' This is what we do when we devise an empirical formula to represent, say, the rate of evaporation from a lake, or when we fit a Fourier series to the diurnal variation of temperatures. It is a humdrum use of mathematics, but essential in a subject such as meteorology, in which, at times, the sheer mass of observational data threatens to obscure the physical processes at work.

The second main use of mathematics in physical science is to make predictions. This involves certain distinct steps, common to all exact sciences. They are :

(i) the formulation of a *theory*, or imaginative hypothesis, concerning the nature of a physical process;
(ii) the setting of an *idealized problem*, involving the application of the hypothesis to a prescribed situation;
(iii) the solution of the idealized problem by mathematics;
(iv) the comparison of the mathematical solution with observations.

It is essential that we recognize clearly the critical nature of step (ii). We cannot solve real problems of physics by mathematics; we can solve only certain ideal problems which have affinities with the real problem. In many problems of physics the agreement between the numbers which appear in the mathematical solution and the pointer-readings of the laboratory instruments is so close that the illusion of having solved a real problem is almost overwhelming. However, even in the simplest relations, such as the equation of state of a gas given above, the agreement with observation cannot be perfect, if only because of the molecular structure of a gas. In meteorology, the gap between the real situation and the ideal problem is much greater than in laboratory physics, so much so that it may appear sometimes that the mathematical meteorologist hardly lives in the same world as his ' practical ' colleague, but the difference is one of degree only, and not of principle.

A science may be regarded as ' exact ' when two conditions can be fulfilled. First, it must be possible to make observations of high precision on the state of a system, and, second, it must be shown that the solutions of certain mathematical problems correspond closely to the numbers indicated by physical measurements. How close the correspondence needs to be is largely a matter of judgment, based on our beliefs regarding the reliability and objectivity of the observations. Obviously, the criteria which apply to, say, spectroscopy are much too severe for meteorology, and I suggest here that we may be well satisfied with a meteorological ' theory ' in which the difference between the observed and calculated values is not more than, say, 10 per cent. In passing, I may add that I know of very few meteorological theories which satisfy this criterion at the present time.

2. The nature of the meteorological problem

In meteorology we are concerned with physical processes taking place in the atmosphere, a shallow ocean of gases held to the surface of the earth by the force of gravity. This layer of gaseous fluid, which hardly extends beyond one or two per cent of the radius of the earth, is subjected to intense radiation from the sun, but relatively little energy is absorbed directly by the air. Much of the radiation is reflected back to space, but a variable amount, sometimes nearly one-half of the total, passes through the atmosphere to warm the surface of the earth. The first picture we have of the atmosphere is thus of a shallow gaseous fluid heated from below, which immediately suggests certain problems in natural convection which, to date, have yet to be solved satisfactorily even for the relatively simple conditions which can be realised in the laboratory. In addition, there is the very significant complication that the convective system is based on the spinning earth.

The main constituents of the atmosphere are the gases nitrogen and oxygen which are always present in states far removed from liquefaction. For the meteorologist, the most important gas in the atmosphere is water vapour. There are three main reasons for this. First, water vapour easily passes to the liquid and solid states (and vice-versa) at terrestrial temperatures. Second, water has very large latent heats, and third, the gas is capable of absorbing and re-emitting radiation from a body at terrestrial temperatures with relative ease. These facts mean that water in the atmosphere supplies an effective means of taking up and releasing heat in enormous quantities. The result is to complicate tremendously the convective system formed by the sun, the atmosphere and the earth's surface. The system can hardly be isolated from the universe as a whole and also is one in which the state of the lower boundary influences, and is influenced by, the condition of the gaseous fluid resting upon it. In other words, the meteorologist has to deal with the vagaries of an open system which is strongly cross-linked. Almost every element of the system reacts significantly with every other, and non-linearity, the bugbear of the mathematician, is the rule rather than the exception.

Climate, or the average condition of the atmosphere over a large area, depends primarily upon a number of stable, enduring factors, such as the regularity of the motion of the earth around the sun and the disposition of land and water over the globe. The phenomenon of weather, that is, the appearance of short-lived large-amplitude deviations from the mean condition of the atmosphere, is no doubt largely dependent on the presence of water in the air and the relative shallowness of the atmosphere. If the earth were the size of a major planet (and thus able to retain a very deep atmosphere) and if life could exist in an atmosphere composed only of the so-called ' permanent ' gases, it is possible that weather itself would cease to exist as a major phenomenon, and with it the need for meteorologists !

Meteorology is often compared, to its disadvantage, with astronomy, but as I have pointed out elsewhere (1951), the analogy is weak and misleading. The typical astronomical forecast, say, of that a solar eclipse, expresses a geometrical property of a relatively simple, closed, stable system which is dominated by one force, gravitation. This is the main reason why astronomical predictions reached a high level of accuracy at a comparatively early stage in the history of European science. Further, it is possible to use a very idealized model, that of a finite universe, with the stars scattered over the surface of a celestial sphere, to describe the apparent motions of the heavenly bodies with complete success. For practical use in navigation, the system is inverted. The astronomer calculates the apparent position of a star at a point on the earth and we use this knowledge to deduce that we are at this position by observing the star. I do not foresee the day when we shall navigate by timing, say, the arrival of a front ! Meteorology, on the other hand, deals

with an open system in which there are many factors of about equal magnitude, operating simultaneously and dependent, to greater or less degree, on each other. Mathematics succeeds easily in spherical astronomy because the passage of the stars in the heavens, 'the army of unalterable law,' is eternal in its regularity, but the weather remains, to poet and scientist alike, symbolic of instability.

3. The mathematical problems of meteorology

'Who can number the clouds in wisdom?' was asked of Job and the question is pertinent even today. The problems of meteorology, however complicated and intractable they may appear, are still those of physics and the task of the mathematical meteorologist is primarily one of setting the problems so that their resolution throws light on the real situation. There is a strong temptation to propound soluble problems in physics which have some relation to atmospheric processes and to call the result 'dynamical meteorology' – a type of academic exercise which is only too familiar in the older texts. The real advances are not made in this way. The essential step lies in discovering the framework of the real problem, after which the idealization, solution and final comparison with observations are relatively easy. It is unprofitable to formulate first an artificial but soluble problem and then to look for occasions when the atmosphere appears to conform.

We may, I think, distinguish two classes of problems in meteorology. In the first class I place those problems which have nearly everything in common with laboratory problems except the possibility of complete control. Evaporation from a free-liquid or permanently saturated surface is typical of this class. In the second class we have problems which have little or nothing in common with laboratory experience, such as those which appertain to the development and movements of the large-scale pressure systems of the middle latitudes. I shall try to show that we may hope to apply mathematics with profit in both classes, but with very different criteria of success.

4. Evaporation

I have selected evaporation as a typical problem of the first type for a number of reasons. In the first place, the problem can be approached in two different but legitimate and profitable ways, both of which are interesting and illuminating. Secondly, there is a real need, felt by the agriculturist and the civil engineer as well as the meteorologist, for a reliable solution. Thirdly, the complete solution has yet to be found (so that there is here an attractive field for the mathematician), but enough has been done to indicate that the problem is tractable.

Evaporation is the process by which water is removed, as vapour, from a surface exposed to the atmosphere. We can approach the problem of its evaluation in two ways :

(i) by an examination of the mechanism of the process, disregarding the economics,
or (ii) by evaluating the energy balance, disregarding the mechanics.

In the first method the problem is treated as one of diffusion, or the forced convection of matter by a turbulent air stream. We may then distinguish three cases : those of the infinite, semi-infinite and finite areas. The infinite area problem means that we are thinking of evaporation taking place from, say, the middle of the ocean, so that the direction of the wind is immaterial. In the semi-infinite problem the wetted area is supposed to have definite boundaries in the direction of the wind, but to stretch indefinitely across wind (this, of course, is merely a mathematician's way of indicating that he is disregarding lateral edge effects). Finally, there is the unsolved problem of an area of any shape.

The semi-infinite area poses a typical boundary-value problem, of a type familiar in the theory of conduction of heat. In brief, it is supposed that there is a strip of liquid or of saturated soil, of finite length downwind and extending indefinitely across wind, over which a wind of known velocity blows steadily. A vapour blanket forms over the surface, representing a condition of dynamic equilibrium; the vapour is removed continuously by the wind but the blanket remains, being renewed by vapour from the surface. The problem is thus to express the rate of removal of vapour from the surface in terms of known quantities, these being chiefly the temperature and nature of the surface, its dimensions and the wind speed at some level near the surface.

This history of the semi-infinite area problem conforms to a familiar pattern. The earliest treatment which I know is that of Sir Harold Jeffreys, published in the *Philosophical Magazine* in 1918, and later studies have followed this lead. In Jeffreys' analysis the problem becomes one familiar in the theory of conduction of heat. It is assumed (i) that the wind is invariable with height, and (ii) by the introduction of an eddy diffusivity K, also independent of height, that eddy diffusion differs from molecular diffusion only in scale. The boundary conditions are all obvious except that which specifies the concentration of water vapour at the wetted surface. Jeffreys assumed this to be constant, a shrewd conjecture to which subsequent laboratory measurements have lent support.

In this work the total evaporation from an area extending a known finite distance downwind is given by an explicit expression involving the wind speed, dimension of the area and the eddy conducitivity K. If K were a true universal constant, knowledge of the wind speed, the relative humidity, temperature of the surface and the size of the area would enable the meteorologist to calculate the amount of water carried over the downwind edge of the area in a given time. I do not know of any detailed comparison of Jeffreys' expression with observations, and today we would not expect to find satisfactory agreement because we know that there is no one value of K which applies to all conditions. However, Jeffreys' analysis brings to light the important fact that in this arrangement evaporation varies with distance from the leading edge, so that the phrase ' rate of evaporation per unit area ' is ambiguous unless the size and position of the reference area is specified. This is not so in the infinite area problem, in which all properties are supposed to be independent of position in a horizontal plane.

In effect, Jeffreys' solution could not be used to make predictions, because of the uncertainty regarding the value of K. It could indicate only the broad features of the process. In 1934 I published an extended treatment of the semi-infinite area problem, based on ideas regarding eddy diffusion which had been developed in the study of smoke clouds. The mathematics followed much the same lines as in Jeffreys' solution, but took into account the variability of wind speed with height and expressed the effect of turbulence, not by a fictitious constant, but in terms of measurable quantities. This solution could be used to make predictions, and soon afterwards Dr. F. Pasquill (1943) carried out a very thorough wind-tunnel investigation. Pasquill's work showed that the measured total of evaporation from small saturated surfaces agreed with the mathematical forecast to within about 10 per cent.

At this stage in the investigation it would be difficult to claim that this work was peculiarly meteorological, for the mathematical treatment and the subsequent verification were all on lines familiar in any physical problem. The typical features of the meteorological problem enter when we begin to consider density gradients in the air stream, as shown in nature by superadiabatic lapse rates and inversions, and the effects of the ' roughness ' of the surface. It is difficult, but perhaps not impossible to simulate such density effects in the laboratory but much more difficult to specify surface irregularities, of the type found in nature, by mathematics. There has been one notable attempt, in

America, to extend the laboratory type of investigation to the meteorological scale. A full account of the Lake Hefner project is not yet avaialble, but in the published preliminary survey (1952) of results it is concluded that two of the theoretical formulae tested ' gave good results ' (the ratio of the average computed evaporation to that observed was about 10 per cent too high in both cases). Meteorologists naturally will reserve judgment on this conclusion until they can examine the full report and will wish to satisfy themselves on the objective nature of the assumptions, but it seems that there is unlikely to be any very large discrepancy between theory and observation.

The study of evaporation is instructive because it illustrates very clearly the way in which laboratory physics of the orthodox type merges into meteorology, but with a substantial loss of precision as it does so. This particular problem, approached in the classical manner, is yielding to research in a fashion with which scientists have grown familiar since the seventeenth century. There can be little doubt that as we discover more about the mechanics of turbulence we shall get better agreement, and that the finite-area problem, which mathematically is very difficult, also will be solved in the years to come, but when we speak of ' agreement ' between theory and observation we must be careful to remember that we are still dealing with a correlation between an ideal problem and a real situation. All observations of physical quantities contain random fluctuations covered by the generic term ' noise,' which limit the precision with which the result can be stated. The mathematical problem effectively relates to an ideal, noise-free system. In meteorology, the ' noise level ' is always much higher than in laboratory physics, and meteorologists must accept as satisfactory agreement between quantities which are, to the laboratory physicist, rather crude averages. This point has been underlined by Sir David Brunt in his *Physical and Dynamical Meteorology*, when he points out that to measure air temperature to a hundredth of a degree centigrade is probably a waste of time because ' the air does not know its own temperature to that degree of accuracy.' This is not to decry attempts to design better instruments, but to emphasize that in meteorology the most sensitive instrument is not necessarily the best for the job in hand.

The alternative way of attacking this problem is one which would hardly recommend itself to a laboratory physicist, but it has considerable merits in meteorology. The energy-balance method consists in first evaluating or estimating all the terms in the transformation of energy except one, that which represents the heat used up in evaporation, which is finally determined as a difference. In symbols,

if F_I is the solar radiation impinging on the surface of the water,

F_R the part reflected,

F_B the outgoing long-wave radiation,

F_H the upward flux from the surface as sensible heat,

F_S the energy absorbed and retained by the water,

we can write the energy balance equation in the form

$$F_E = F_I - F_R - F_B - F_H + F_S$$

where F_E is the heat used up in evaporation. The main uncertainty lies in estimating F_H, because this implies a knowledge of the diffusion of heat by conduction and turbulence.

This method has been used with considerable success by Dr. H. L. Penman (1950) and others. It is essentially a climatologist's approach and obviously applies only to

infinite areas. We may regard it as a bridge between the laboratory type of experiment in which, by rigid controls, extraneous effects are eliminated or reduced below the noise level, and the true meteorological trial, in which control is usually impossible and full and careful observation of all the relevant factors must be accepted as indispensable.

Evaporation is, I think, typical of a large class of problems in which we may look forward with confidence to ultimate success. In such studies we prescribe the state of the atmosphere and ask how this affects certain physical processes. Provided that we take a sensible view of the desirable accuracy, there seems to me to be no reason why we should not look forward ultimately to writing 'Solved' across most, if not all such problems. It is otherwise with problems arising in the phenomenon of weather, which I shall deal with next.

5. Mathematics in the synoptic problem

The central problem of meteorology is that of forecasting weather. Attempts to bring this within the discipline of mathematics have followed, in the main, three lines :

(i) by the study of 'model' atmospheric disturbances,
(ii) by statistics,
(iii) by the equations of hydrodynamics.

The depressions and anticyclones of the middle latitudes have certain obvious affinities with motions studied in the classical hydrodynamics of revolving fluids. Analyses of synoptic situations based on such considerations flourished in the early years of this century, for example, Rayleigh's attempt to compare the middle-latitude depression with a rotatory disc of incompressible fluid with fluid withdrawn from the centre, the analysis of a tropical cyclone as a simple $vr = constant$ type of vortex, or the 'cartwheel' depression of Shaw. Such mechanical models, if I may use the term, are now of historic interest only. They represent an excessive idealization, for essentially they are two-dimensional models given a three-dimensional appearance.

The three-dimensional character of the middle-latitude disturbance begins to emerge, however, in the more sophisticated treatment which has been Norway's outstanding contribution to meteorology. The concept of the evolution of the depression as a development of wave-like perturbations of the polar front gave forecasting its greatest impetus. We know now that the true Bjerknes depression is somewhat rare and that the sequence of events described in the earlier papers represents a very considerable idealization of what actually occurs, but even so, we have every reason to be grateful to the Bergen school for having taught us, by precept and practice, the value of air-mass analysis in routine forecasting.

The statistical approach has attracted, possibly, more workers than any other, but today I doubt if anyone (apart from a few enthusiasts) would claim that the results obtained are commensurate with the effort expended or that the method merits further serious study. This, of course, is not to say that statistical theory has no place in meteorology. There are many questions about the atmosphere which can be answered with assurance only by the application of the statistical method, and climatology could hardly be contemplated without assistance of this kind. But it seems to me that the search for empirical periodicities and correlations is not only unsound but dangerous, in that it tends to create a state of mind approaching that of the 'true believer,' who sees examples to support his creed in every case he examines. How badly such methods stand up to objective examination is well illustrated in Sir Gilbert Walker's devastating criticism of the use of 'symmetry points' in weather prediction, and as regards the possibility of using harmonic analysis in forecasting generally I feel I cannot do better than quote two of our leading experts, Dr. C. E. P. Brooks and Mrs. H. Goldie, who

conclude their stimulating book on meteorological statistics with the verdict that ' Periodicities in meteorological phenomena (apart from annual and diurnal waves) are rarely, if ever, sufficiently persistent to be of use for prediction.' The application of statistical theory to pressure-time series for the purpose of forecasting weather would appear to be a misuse of a mathematical tool. The prime purposes of statistical theory are to examine data for internal consistency and to weigh the evidence for a previously formulated hypothesis, and in meteorology, as in any other science, it is essential that the research have a sound physical basis before any statistical approach is attempted.

In the ' mechanical model ' and ' statistical ' methods we have two widely different approaches. There is, however, a middle way which seems to me to hold out considerable hope, although at first sight it would appear to involve insuperable difficulties. This is the direct attack on the problem by means of the equations of hydrodynamics. The essence of the modern method is contained in L. F. Richardson's strange but stimulating book *Weather Forecasting by Numerical Process*, published by the Cambridge University Press in 1922. In this, Richardson, with the *Nautical Almanac* to inspire him, advanced the proposition that it is possible, in theory, to remove completely the subjective element in forecasting and to substitute an entirely mathematical approach. Those who have read this extraordinary book will not fail to remember how Richardson, with his puckish humour, envisaged the Meteorological Office of the future as something like the Albert Hall, with the boxes, circles and galleries occupied by hordes of computers and in the centre the chief mathematician, the conductor of this monstrous orchestra, equipped with a device which played a blue light on those who were keeping up, and a red light on those who were falling behind, in the daily race with the weather ! He estimated that to race the weather for the whole globe would require 64,000 computers.

I think that today few meteorologists would admit to a belief in the possibility (let alone the likelihood) of Richardson's dream being realised. My own view, for what it is worth, is definitely against it. It is not so much the complexity of the situation which appals the mathematician, but the fact that the problem may be indeterminate. There are two main reasons for suspecting this to be the case. I have indicated earlier how meteorology differs from astronomy, and to be more specific, it seems that we must admit that at times the initial conditions, as we know them, may be insufficient to determine an unique solution. By this I mean that the atmosphere is so nearly unstable that disturbances which are below the threshold of observation at one time, or which are so small initially that they are removed in the necessary process of smoothing, can grow exponentially and affect the distribution significantly at some later time (this, in effect, is putting into general language what the mathematician means by ' instability '). The second reason why we may suspect genuine indeterminacy (as distinct from mere complexity) is that we do not yet know how far unpredictable astronomical events, such as bursts of cosmic radiation, sunspots, meteoric showers, may affect the macro-processes of the atmosphere. If they are capable of causing significant changes, we must admit that weather is really indeterminate, for some at least of these phenomena are the result of occurrences which took place aeons ago deep in the remote universe.

All this seems to point to the conclusion that mathematics cannot play an effective part in weather forecasting, but I think that this is being unduly pessimistic. Even if Richardson's concept of a *Nautical Almanac* of the atmosphere must remain a dream, we can still use mathematics with profit, but only if we ask the right kind of question. In routine forecasting, the weather prognosis is built around the time extrapolation of the existing pressure fields. In current British practice the extrapolation is made definite by the production of a prebaratic chart, but the process is highly subjective. In deciding how the air masses will rearrange themselves in the course of the next 24 hours, the

forecaster may be guided by the properties of a model, such as the Bjerknes depression, by considerations of the topography of the pressure surfaces, or by a set of private empirical rules which constitute his ' experience.' Whatever method is adopted, it is evident that good judgment and long practice are essentials in this difficult art. In my view the mathematical method should be regarded as a means of indicating the most *probable* line of development. It seems that we should be unwise to expect more than this, at least for some years to come. If it could be shown that the pressure fields predicted by mathematics bear a strong qualitative resemblance to the actual fields on a large majority of occasions, much of the subjective element in weather forecasts would be eliminated.

To illustrate this point let us consider what has been achieved so far in this country. Work on numerical forecasting is in progress in the Napier Shaw Laboratories of the Meteorological Office at Dunstable, under the direction of Mr. J. S. Sawyer, the Assistant Director in charge of Forecasting Research. Although as yet very few calculations have been carried through to the ' 24-hour forecast ' stage, enough has been done to show some of the potentialities of the method.

Ideally, any system of mathematical prediction should be based on conditions over the whole globe, but in practice, for a 24-hour forecast, it is sufficient to restrict the calculations to the movement of air masses over a rectangle covering part of the eastern Atlantic and part of western Europe. With the present network of reporting stations we know tolerably well the distribution of air pressure at the surface and at some considerable height in the troposphere (say, the 500-mb level) within this rectangle at any one time. If we give the atmosphere a conventional ' lid,' say at the tropopause, and assume that conditions do not change (or change according to some prescribed law) at the boundaries of the rectangle, the problem amounts to tracing the movements of the air inside the box formed by the rectangle, the vertical walls and the ' lid ' in a period of, say, 24 hours. This is the classical second-order partial differential equation problem with given initial and boundary conditions, which usually has a unique solution. The difficulty of the meteorological problem lies mainly in the fact that the equations are non-linear and if we try to include all the factors – such as viscosity, conduction of heat and thermodynamics – the problem becomes too complicated to be tractable. By selecting certain processes as fundamental and ignoring others we can formulate a soluble problem with a unique solution which, it is hoped, will have strong affinities with the real situation. In other words, we set up a model atmosphere which obeys certain simplified forms of the laws of nature, but we are not restricted to a model of an anticyclone or depression in the sense used earlier. Without going to the extreme generality of Richardson's attempt we can yet avoid the pitfall of a rigid ' theory.'

In the Sawyer-Bushby method, the atmosphere is supposed to be contained between two isobaric surfaces across which air flow is deemed to be negligible. (In the calculations these surfaces are fixed as 1,000 and 200 mb, corresponding roughly to sea level and the tropopause, respectively). A truly barotropic atmosphere being too restrictive some features of a baroclinic atmosphere are admitted. The state of motion in the model is studied by considering the topography of an intermediate isobaric surface (say 500 mb) which gives effectively the geostrophic wind. The thickness, or relative topography of the surface, is related to the thermal wind, which is assumed to be constant in direction in any vertical column.

(It is perhaps worth while pausing here to consider a difficulty peculiar to meteorology. In middle latitudes the geostrophic wind is an approximation to the actual wind, but in a geostrophic system the balance is maintained without vertical motion, which in effect eliminates the weather. The fact that normally vertical velocities are too small to be measured may be expressed more dramatically by the statement that meteorology suffers

because the geostrophic wind, the first approximation to the actual wind, is too good !)

Thus in this particular model, like many others, is a compromise. For some time to come meteorologists must be prepared to accept models which are not strictly logical, in that there is an arbitrary selection of certain features to be retained, while others are rejected, not necessarily because they are small but because their retention would make the model unworkable. As in mathematical physics generally, such models are idealizations, useful but not easily justified on strict mathematical grounds.

In the Sawyer-Bushby model computations are carried out in terms of two parameters, the height (h_m) of the central pressure level of a selected isobaric surface and the thickness (h') of the layer. The analysis leads to two second-order partial differential equations for the instantaneous rates of change of h_m and h'. If these equations are solved at a time t_0, the values of h_m and h' at time $t_0 + \delta t$ can be found from $\partial h_m/\partial t$ and $\partial h'/\partial t$ by a simple finite-difference extrapolation. This process can be repeated n times to give forecast height and thickness patterns over the rectangle at times $t_0 + n\delta t$. In the actual trials, the equations are solved numerically, by an electronic digital computing machine, for a grid of 192 points. This process, when carried out on a computer of relatively small storage capacity, requires 8 hours of machine time to produce a 24-hour forecast. With more advanced machines this can be reduced to 1 or 2 hours of machine time. Experience shows that the separation of points in the network should not be more than about 150 mi, and the integrations need to be carried out in steps of about 1 hour or less. These requirements underline the need for the maintenance of a close network of reliable upper air and surface observations if this type of work is to be given a fair chance.

The details of the initial tests with the Sawyer-Bushby model will be published, I hope, in the near future and here I shall not attempt to discuss further the somewhat complicated mathematics involved. We are more concerned, at the moment, with the implications and potentialities of the method. In the first place, it is important to realize that there is no attempt to produce a forecast of weather. The mathematicians, very wisely, are limiting their attention to an examination of the pressure and motion fields over a relatively small part of one hemisphere, in the middle latitudes. In other words, the method results in the production of a prebaratic. Even if the method were infallible (which it is not) the services of the meteorologist, as distinct from the mathematician, would still be required to ' put in the weather,' a phrase which covers what may well be the most difficult part of the whole process of forecasting. Secondly, it is as well to realize that the preparation of the model implies the belief that the selected processes are truly dominant, while others may be ignored. The hope is that on a majority of occasions this selection will be justified by the results, but the answer must always be in terms of probabilities, and in this sense the machine-made prebaratic will always be subjective. It is subjective also in another way. The initial data fed into the machine consist of values of the pressure at the two selected levels over a grid of about 200 points. This means that the initial pressure distribution has to be bulit up by a process of interpolation and extrapolation from the observed values. The number of observations available for this purpose is small in the upper air and over the sea. The initial step is the drawing of the pressure field by the meteorologist in the usual fashion, from which the grid values are read off. Over the North Atlantic and the less densely populated northern regions the evolution of the pressure patterns frequently involves a good deal of judgment, so that it cannot be claimed that the initial data are entirely objective. Where the network of observations is dense, no doubt an objective mathematical method of deducing the grid values can be devised, but if the observations are few (as over the North Atlantic at present), experience must be used, which means that a human forecaster must prepare the initial map.

Finally, it will be the responsibility of the forecaster, drawing on his experience, to accept or reject the answer provided by the mathematicians and he must always recognize that the subjective element has not been eliminated by the mathematics but removed a stage further back, to the initial data and the postulates of the model.

The Sawyer-Bushby scheme has been tried, so far, on a few occasions only. The results are encouraging. The prebaratics produced by the machine for the surface and the mid-troposphere have reproduced the main kinematical features of the real situations. As prebaratics they are about as good as those which were drawn by experienced forecasters on the dates in question. Thus while it is far too early to claim that the value of the method is established, there are good grounds for continuing the research.

6. Conclusions

Our review of the progress of meteorology towards the status of an exact science has, I hope, enabled us to draw some fairly definite conclusions. We have seen that, broadly, there are two main classes of investigations which arouse hopes of quantitative treatment. The first group, typified by the problem of evaporation, is concerned with the part played by the atmosphere in physical processes which are well known in the laboratory. The peculiar feature of the meteorological problem, as opposed to the laboratory investigation, is that control is impossible and observation has to take its place. Micrometeorology, the mathematician's paradise, is preoccupied with such investigations, and it is no great act of faith to predict that some day such problems will be ' finally solved.' I think that we may be even more definite than this, and claim that already, in these fields, meteorology may be regarded as an exact science.

In the second class of investigations we are concerned with the way the atmosphere behaves, quite apart from its effects. Here the problem is quite different from those of the laboratory and it is difficult to believe that any workable model of the atmosphere will yield results which compare in accuracy with, say, those of spherical astronomy. What we have discovered about large-scale atmospheric motions in the lifetime of our science – about a century – indicates that we have to deal with a system which will not yield fully the type of analysis which is common in the truly exact sciences. Our answers must always be expressed as probabilities.

The fact that a weather forecast is essentially a statement of chances is not fully appreciated by the non-meteorologist and I believe that some of the distrust with which the forecasts are regarded can be attributed to this misunderstanding. Here a Society like ours, which counts among its members both professional meteorologists and laymen, can do much to help. The task of the meteorologist does not finish with the composition of the forecasts – there is still the very important problem of conveying the results of the analysis to the public in simple and straightforward terms. Only when this is done can we say truly that we have accomplished our task.

References

Jeffreys, H. 1918 *Phil. Mag.*, **35**, p. 270.

Pasquill, F. 1943 *Proc. Roy. Soc.* A, **182**, p. 75.

Penman, H. L. 1950 *Quart. J. R. Met. Soc.*, **76**, p. 372.

Sutton, O. G. 1934 *Proc. Roy. Soc.* A, **146**, p. 701.

1951 *Weather*, **6**, p. 291.

Washington, Dept. of Interior 1952 ' Water-loss investigations,' Vol. 1 – Lake Hefner Studies, *Tech. Rep. U.S. Geological Survey*, Circ. No. 229.

551.511 : 551.558.1 : 536.2

Heat convection and buoyancy effects in fluids

By G. K. BATCHELOR
Trinity College, Cambridge

Symons Memorial Lecture, delivered 7 April 1954

There is no need for an explanation, to this audience, of the importance of heat convection and buoyancy effects in meteorology. You will all appreciate, much better than I do myself, that buoyancy forces play a dominant role in determining atmospheric motions and that they provide an essential mechanism for one part of the energy cycle of the atmosphere. The simple fact that an element of air which is warmer than the surrounding air rises under the influence of gravity, is responsible for a host of meteorological phenomena, and enters into considerations of the general circulation of the atmosphere, and at the other extreme of scale, of the best place in a garden for strawberry plants. And, aside from the preoccupations of the meteorologist, who has not marvelled at the majesty and beauty of a cumulus cloud ?

I could not hope to touch on all the meteorological manifestations of heat convection and buoyancy effects in the course of this lecture : nor should I be competent to do so, since my acquaintance with meteorology is very limited. What I propose to do is to look briefly at the subject of free convection in fluids, from the point of view of a theoretical physicist, and to give an account of some of the effects or problems that seem to be fundamental, bearing in mind the meteorological situation. Free convection – that is, convective motion that is due entirely to buoyancy forces – has not been studied as intensively as most other branches of fluid dynamics, possibly because it plays little or no part in the subject of aeronautics which has been responsible for the direction of so much of the research in fluid dynamics. Meteorology is the field of applied science that has greatest interest in the subject of free convection, and I suspect that hitherto the liaison between meteorologists and fluid dynamicists in the laboratory has not been close enough for one group to stimulate the other. Possibly fluid dynamicists have been blind to the many interesting problems of convection waiting to be solved ; possibly meteorologists have not been aware that many natural manifestations of free convection can be reproduced in the laboratory. There are signs of increasing contact between the two groups, and one of the aims of this lecture is to describe the common ground that exists between them.

1. General remarks about problems of free convection

The essential feature of problems of free convection is that the distributions of velocity and temperature are interdependent. The velocity of the fluid is due entirely to the action of buoyancy forces arising from variations in the density, and this same distribution of density, or, equivalently, of temperature, is modified as soon as the fluid moves. Thus the distributions of velocity and density cannot be considered separately; they are both part of the problem and must be considered together. This fact, and the non-linearity of the equations of motion of the fluid, make problems of free convection very difficult, and very few exact mathematical solutions of the governing equations are known. Mathematically, problems of free convection are among the most intractable in fluid dynamics, and the meteorologist must seek help more from the experimenter than from the mathematician.

However, although exact analytical formulae will not often be available, a good deal can be learnt from a partial theoretical analysis of problems of free convection. In particular, dimensional analysis will carry us a long way. Dimensional analysis proves to be an exceedingly powerful weapon, although I should add the warning, based on bitter experience of my own as well as a critical study of other people's work, that it is also a blind, undiscriminating weapon and that it is fatally easy to prove too much and to get more out of a problem than was put in to it.

It will be instructive to look at the general form of the governing equations, although I do not propose to make detailed use of them. Provided it can be assumed that the variation of the absolute temperature T throughout the fluid is small compared with T itself, the equations governing the distributions of temperature T and velocity (u, v, w) in problems of free convection in a perfect gas can be written in the following approximate form (see *Modern Developments in Fluid Dynamics*, O.U.P., 1938, Chap. 14)

$$\text{Heat equation :} \qquad \left(\frac{\partial}{\partial t} + u\frac{\partial}{\partial x} + v\frac{\partial}{\partial y} + w\frac{\partial}{\partial z}\right)\frac{T - T_0}{T_0} = \kappa\left(\frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2} + \frac{\partial^2}{\partial z^2}\right)\frac{T - T_0}{T_0}$$

$$\text{Momentum equation :} \quad \left(\frac{\partial}{\partial t} + u\frac{\partial}{\partial x} + v\frac{\partial}{\partial y} + w\frac{\partial}{\partial z}\right)(u, v, w) = -\frac{1}{\rho_0}\left(\frac{\partial}{\partial x}, \frac{\partial}{\partial y}, \frac{\partial}{\partial z}\right)p + \frac{T - T_0}{T_0}(0, 0, g) + \nu\left(\frac{\partial^2}{\partial x^2} + \frac{\partial^2}{\partial y^2} + \frac{\partial^2}{\partial z^2}\right)(u, v, w)$$

$$\text{Continuity equation :} \qquad \frac{\partial u}{\partial x} + \frac{\partial v}{\partial y} + \frac{\partial w}{\partial z} = 0.$$

Here z is the vertical coordinate (upwards, positive), ρ_0 and T_0 are the density and temperature at some given point in the fluid, $\nu = \mu/\rho_0$ is the (uniform) kinematic viscosity, and $\kappa = k/c_p\,\rho_0$ is the (uniform) thermal diffusivity. It will be noted that $(T - T_0)/T_0$ occurs in the momentum equation in association with the gravitational acceleration g, and that this is so in all the equations if both sides of the heat equation are multiplied by g. The physical significance of this fact is that temperature enters into the problem solely through its effect on the excess weight (more precisely, the excess weight divided by ρ_0) of the fluid. Thus in all dimensional arguments we must take care to regard $g(T - T_0)/T_0$ as a single variable.

Representative values of some of these physical quantities will be supplied by the data of a problem, i.e. by the conditions at the boundaries and at the initial instant of time. For instance, the difference between the temperature at a conducting boundary and the temperature of the fluid at some distance from it will often be specified; on the other hand the velocity will not be specified, because the motion of the fluid develops wholly as a result of the thermal boundary conditions. Of course, in nature it often happens that a forced wind, due to some extraneous factor, is superimposed on the convection due to buoyancy, but in the interests of simplicity I am excluding such cases. Sometimes the heat flux across a certain boundary will be given, and in other cases it will be unknown like the velocity. It will be convenient always to write the flux of heat in the form

$$(c_p\,\rho_0\,T_0/g)\,F,$$

where F has dimensions (velocity)^3 $\times$ length; if this heat is given to fluid which is initially at the standard density ρ_0 and temperature T_0, $\rho_0 F$ is the weight *deficiency* bestowed on a given large volume of the fluid in one second.

The dimensionless combinations that can be formed from the physical quantities of which representative values are given as part of the boundary conditions are as follows :

$$\text{Prandtl number } \sigma = \nu/\kappa, \quad \approx 0{\cdot}7 \text{ for air},$$

$$\text{Rayleigh number } A = g\,(T_1 - T_0)\,L^3/T_0\,\kappa\nu,$$

$$\text{Nusselt number } N = F\,T_0/(T_1 - T_0)\,\kappa g L.$$

where L is a representative length and $(T_1 - T_0)$ is a representative temperature difference. The Richardson number, which can be regarded as a measure of the importance of buoyancy forces relative to inertia forces, does not enter here, since inertia forces depend on the velocity and no representative value of the velocity is given by the data of the problem. The meaning of the Rayleigh number is readily seen from a consideration of the magnitude of the velocity V that would be set up by buoyancy forces in steady laminar flow. For the magnitude of the viscous force of retardation is then of order $\nu V/L^2$ (provided L is a measure of the scale of the velocity variation) and when this is balanced by a buoyancy force of order $g\,(T_1 - T_0)/T_0$ we have

$$V \sim g\,(T_1 - T_0)\,L^2/T_0\,\nu = A\kappa/L,$$

so that

$$A \sim VL/\kappa.$$

Thus A is analogous, and numerically roughly equal (in the case of air), to the Reynolds number of the steady laminar motion set up by the buoyancy forces.

In the important case of turbulent flow, the influence of molecular diffusion of momentum and heat may be neglected, except in considerations of the small-scale components of the motion and of conditions at points close to a rigid boundary. Thus for turbulent flow, in general, the Prandtl and Rayleigh numbers lose their significance; this is because the Rayleigh number is already so large that a further increase in A does not have any effect on the flow. In cases of turbulent flow we shall need to consider only the effect of variation of the Nusselt number, unless considerations of conditions close to a rigid boundary – where heat conduction and viscous forces are never unimportant – are involved.

Before turning to a consideration of particular cases of free convection, it may be useful to recognize the different types of problem that are likely to be of interest to the meteorologist. There will first be the obvious division into cases of laminar flow and cases of turbulent flow, and in view of the interpretation of the Rayleigh number as analogous to a Reynolds number it is clear that, for given boundary conditions, a convective motion will be turbulent when the Rayleigh number exceeds a certain critical value. Another important division, separating flow systems with quite different features, is into cases in which the source of heat is concentrated into a point or a line, and cases in which the source is distributed over an area. True point sources do not occur in nature but, if the linear dimensions of the source are small compared with the scale of the whole flow, the fluid will behave approximately as though the source were a point. Then there will be cases in which the supply of heat to the fluid is maintained steadily, and, at the other extreme, cases in which the supply lasts for a short time only so that the appropriate boundary condition is that a given amount of heat is released at a given time. All of these different cases have their interesting properties, and I propose now to consider a number of special problems which will illustrate these properties.

2. Problems of free convection due to discrete sources at a horizontal boundary

(a) *Maintained point source : laminar flow*

The rising plume of smoke from a cigarette provides the commonest example of laminar flow due to a steady point source of heat.* The plume widens as it rises, owing to the molecular diffusion of heat and momentum in the lateral plane, and at a height of one or two feet from the source becomes unstable, and turbulent motion develops. Now when a flow pattern develops from a single point, it is a good plan to look for a *similarity* solution of the governing equations, that is, a solution such that velocity and temperature each have similar distributions at different distances from the source and such that their magnitudes vary as some power of z; if such a solution can be found mathematically, it is fairly certain to be the solution corresponding to what is observed experimentally. Thus we assume that the vertical velocity and the temperature depend on position in the following manner :

$$w = z^m \times \text{func}\,(r/R),$$

$$g\,(T - T_0)/T_0 = z^n \times \text{func}\,(r/R),$$

where r is radial distance from the vertical line through the source, and R is a measure of the radius of the heat plume at height z and is also assumed to be proportional to some power of z, i.e.,

$$R \propto z^l$$

The unknown powers l, m, n can then be determined from the form of the governing equations or from simple consequences of these equations. First, the motion is steady so that the flux of excess heat (i.e. the heat over and above that contained by the fluid in its ambient state) across all horizontal planes is the same; that is,

$$\int_0^\infty wg\left(\frac{T - T_0}{T_0}\right) 2\pi r\, dr = F$$

for all z, where $(c_p \rho_0 T_0/g) F$ is the rate at which heat is released by the source. This relation can be satisfied only if

$$2l + m + n = 0.$$

Second, the various terms in the equation of momentum must all depend on z in the same way;

$$w^2/z \left(\begin{matrix}\text{total}\\ \text{acceleration}\end{matrix}\right) \sim g\,(T_1 - T_0)/T_0 \left(\begin{matrix}\text{buoyancy}\\ \text{acceleration}\end{matrix}\right) \sim \nu w/R^2 \left(\begin{matrix}\text{viscous}\\ \text{retardation}\end{matrix}\right)$$

so that

$$2m - 1 = n = m - 2l.$$

Hence

$$l = \tfrac{1}{2}, \quad m = 0, \quad n = -1.$$

After allowing for the fact that w and $g\,(T - T_0)/T_0$ can depend only on z, r, F, κ, ν, we find the general form of the distributions of velocity and temperature to be

* Laminar flow hardly ever occurs in the atmosphere, but laminar flow due to a point source is considered here because it provides a very good illustration of the power of dimensional arguments when a similarity solution exists.

$$w = \frac{F^{1/2}}{\nu^{1/2}} \times \text{func}\left(\frac{rF^{1/4}}{z^{1/2}\,\nu^{3/4}}, \sigma\right),$$

$$g\,(T - T_0)/T_0 = \frac{F}{\nu z} \times \text{func}\left(\frac{rF^{1/4}}{z^{1/2}\,\nu^{3/4}}, \sigma\right),$$

$$R = \frac{z^{1/2}\,\nu^{3/4}}{F^{1/4}} \times \text{func}\,(\sigma).$$

It turns out that it is possible to go further, and Yih (1951) has determined the above unknown functions, for the special cases $\sigma = 1$ and $\sigma = 2$, by solving the governing equations. The solution has the expected feature of a concentration of the momentum and heat in the neighbourhood of the vertical line through the source. The Reynolds number corresponding to the flow at a cross-section of the plume is proportional to $z^{1/2}\,F^{1/4}\,\nu^{-3/4}$, so that at a certain height the Reynolds number will exceed the critical value for stability, and turbulent motion will develop. According to the results of some experiments by Yih, this height is approximately equal to $10^5\,\nu^{3/2}\,F^{-1/2}$.

(b) *Maintained point source : turbulent flow*

A cigarette is a comparatively feeble source compared with objects exposed to sunshine at the earth's surface, such as rocks, house-roofs, areas of concrete, so that the flow due to natural point sources will usually become turbulent at small distances from the source. Again we look for a similarity solution, with the expectation that such a solution, if it exists, will describe the observed quantities at distances from the source large compared with the linear dimensions of the source. The effects of molecular diffusion are negligible in this kind of turbulent flow, so that the average velocity and temperature can depend only on the position variables, r, z, and the quantity F measuring the flux of heat from the source. In these simple circumstances, dimensional analysis leads immediately to the following results :

$$\overline{w} = \frac{F^{1/3}}{z^{1/3}} \times \text{func}\,(r/R),$$

$$g\,\overline{(T - T_0)}/T_0 = \frac{F^{2/3}}{z^{5/3}} \times \text{func}\,(r/R),$$

$$R = \alpha z,$$

where α is a constant. The plume thus has a conical form, with apex at the source, unlike the paraboloidal laminar plume. Near the source, the velocity and temperature become very large, according to this solution, but it must be remembered that the solution does not apply close to the source unless it is strictly a point.

It will be noticed that the mass flow across a section of the plume is proportional to $z^{5/3}$. To provide this increasing mass flow there must be entrainment of stationary air at the edge of the plume and the required horizontal mean velocity of inflow across a circle of radius R must vary as $z^{-1/3}$. Thus the mean inflow velocity is proportional to the mean vertical velocity, and the ratio of the two will determine the angle of the conical plume.

For the determination of further details of the flow it is necessary to know something about the structure of the turbulence at difference positions across the plume. Schmidt (1941), who was the first to consider this problem, gave an analysis of the mixing-length type, but I do not think such an analysis can be regarded as satisfactory at the present time. Yih (1951) – see also Rouse, Yih and Humphreys (1952) – has made measurements

in a room 25 ft wide and 11 ft high, using a gas burner as a source of heat and a thermocouple and small vane anemometer to record the mean temperature and velocity, and has found that the distributions in a horizontal plane can be fitted quite well by the Gaussian curves

$$\bar{w} = 4{\cdot}7 \frac{F^{1/3}}{z^{1/3}} \exp(-96\, r^2/z^2),$$

$$g(\overline{T - T_0})/T_0 = 11{\cdot}0 \frac{F^{2/3}}{z^{5/3}} \exp(-71\, r^2/z^2).$$

Thus if the radius of the plume R is defined as the value of r at which the mean temperature falls to $1/e$ of its value on the axis, we find $R = 0{\cdot}12\, z$, and the semi-angle of the conical plume is 7°. The corresponding streamlines and isotherms have been calculated by Rouse, Yih and Humphreys (1952) and are reproduced in Fig. 1. These measurements by Yih confirmed the expected dependence of $\bar{w}$ and $(\overline{T - T_0})/T_0$ on z, as also did the earlier measurements by Schmidt (1941). Some recent measurements by Railston (1954) seem to show that the mean temperature falls off with z as a power intermediate between the value -1 appropriate to laminar flow and the value $-5/3$ appropriate to turbulent flow, but in these experiments it was not clear that the flow was decisively of one kind or the other.

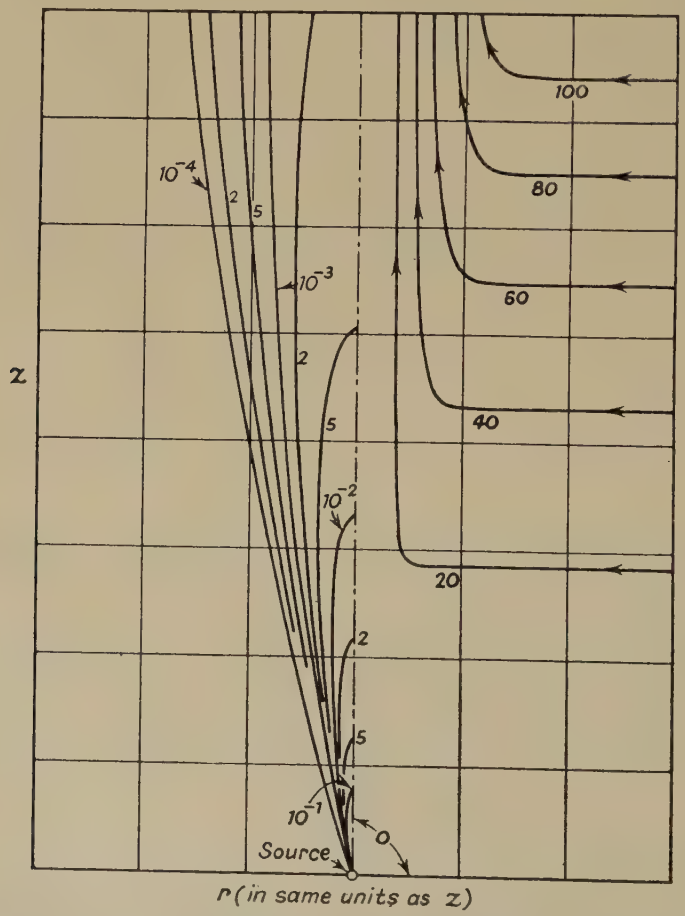


Figure 1. Mean isotherms and streamlines for the turbulent convection due to a maintained point source (after Rouse, Yih and Humphreys 1952). The numbers on the isotherms are relative values of $(T - T_0)/T_0$ and those on the streamlines are relative values of the Stokes stream function.

(c) *Maintained line source : turbulent flow*

The analysis for the turbulent flow due to a line source of given heat output per unit length proceeds, with the assumption of a similarity solution, in an exactly similar way. The side of the plume is again found to be straight, so that it has the form of a wedge, whose angle is again a measure of the rate of entrainment. The mean velocity is found to be independent of z, and the mean temperature to vary as z^{-1}. Measurements of the distributions of mean velocity and temperature over a horizontal plane have been made by Humphreys (see Rouse, Yih and Humphreys 1952), and the following expressions, again of Gaussian form, were found to fit the measurements quite well :

$$\overline{w} = 1{\cdot}80\, F^{1/3} \exp(-32\, x^2/z^2),$$

$$g\,\overline{(T - T_0)}/T_0 = 2{\cdot}6\, \frac{F^{2/3}}{z} \exp(-41\, x^2/z^2),$$

where x is the coordinate across the plume and F now refers to the flux of heat from unit length of the source. If the edge of the plume is defined as the place where the temperature falls to $1/e$ of its value at the centre, the semi-angle of the wedge bounding the plume is here 9°.

An interesting application of the results for a line source has been made by G. I. Taylor (unpublished), in a consideration of the effect on water waves of a curtain of air bubbles rising from a perforated pipe some distance below the surface of the water. It appears that oncoming water waves are damped considerably by the opposing horizontal outflow which is set up near the surface by the rising column over the pipe, and the device is of interest in view of its possible use as a breakwater. The point that is of interest here is that the flow due to a line source of heat and the flow due to a line source of small air bubbles in water are identical since they are both due essentially to sources of weight deficiency. Air bubbles are not diffused by molecular motion as heat is, of course, but they will be dispersed by turbulence in the same way. Provided the source strengths are suitably related, i.e., provided they refer to the same flux of weight-deficiency, the experimental results described above can be used to determine the mass flow above the source and the outflow of water at the surface caused by the rising air bubbles.

Another application of the line-source flow, probably well known to meteorologists, has been made to the problem of clearing fog from aerodromes. During the war, lines of burners were placed alongside aerodrome runways in an endeavour to heat the air and clear the fog in a tunnel large enough to allow an aeroplane to land safely. Several interesting features of convective flow patterns were revealed in the course of the F.I.D.O. investigations, two of which may be mentioned briefly here. The first concerns the flow due to two parallel lines of burners, one on each side of a runway. At first sight, it might be thought that the velocity and temperature distributions would be given approximately by a superposition of the separate distributions for each line source. But, in fact, this is not the case because it is impossible for both plumes to entrain air from the closed region between them. Instead, the presence of the horizontal boundary causes one line source to have a marked effect on the flow due to the other, and the two sources produce a convection pattern that is very similar to that produced by a single source midway between them. Measurements showing this effect very clearly have been made by Rouse, Baines and Humphreys (1953), and are reproduced in Fig. 2. It was to be expected that the flow would look like that for a single line source at distances large compared with the distance between the two sources, but it appears that this is approximately true everywhere. This same effect probably occurs whenever a source is distributed more or less symmetrically over a finite area of the boundary. Thus a heat source distributed uniformly over a circle – or an annulus – probably produces a convection pattern similar to that

due to a point source of the same total strength. As a consequence, a thermal upcurrent in the atmosphere may not always have a position suggested by the local nature of the earth's surface; a distributed source of finite extent probably gives rise to only one thermal and this will be located above the effective centre of the area acting as a source.

The second interesting feature revealed by the work on F.I.D.O. concerns the flow due to a line source in a side wind. I have mentioned that for an isolated line source the mean velocity is independent of z, and the required mean inflow velocity at the side of the plume is likewise independent of z. The measurements made by Humphreys (see Rouse, Yih and Humphreys 1952), require the mean inflow velocity to be 0·16 of the maximum vertical velocity at the same level. Thus there is already a uniform side-wind on each side of the plume, and always towards it. It seems very likely that in the first approximation the effect of a weak external wind, of the same speed in all heights, is simply to superpose a uniform velocity on the convection pattern, so that the plume is slightly inclined to the vertical. But a curious situation arises when the external wind has the speed $0{\cdot}16 \times \bar{w}_{\max}$, for then the inflow velocity on the leeward wide of the plume is exactly cancelled. For stronger external side-winds, it is clear that simple superposition is not possible because there is now a horizontal wind blowing right through the jet and this would carry the heat away from the plume. There is, however, the definite suggestion that for such strong side-winds the plume is not detached from the boundary on the leeward side, and observations by Rouse (1947) and by Rankine (1950) in large wind-tunnels, have shown that this is the case. Theories of the turbulent convection for this latter case have been developed by Rouse (1947) and by G. I. Taylor (unpublished). The case of a point source is probably rather different and a side-wind is unlikely to prevent the plume from leaving the ground.

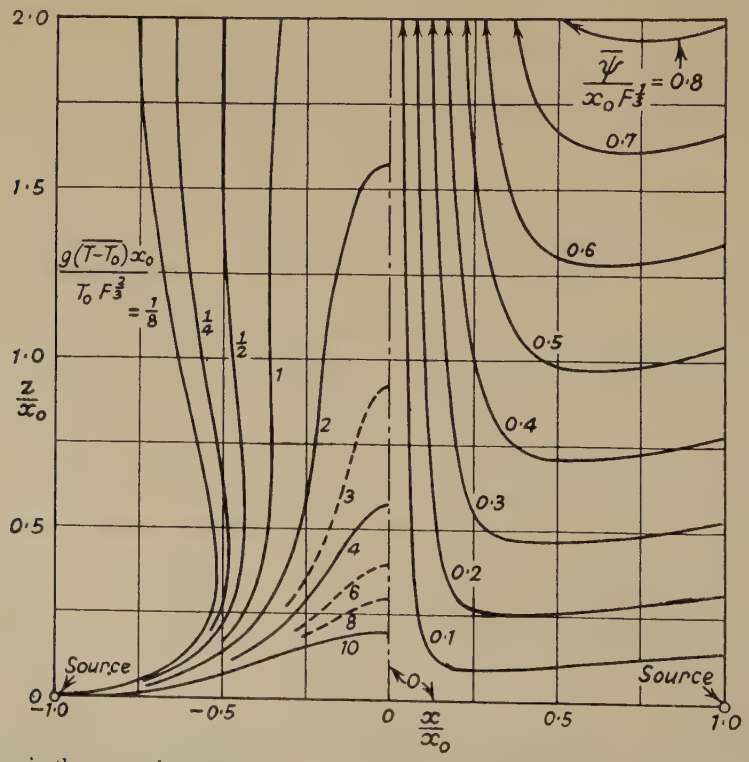


Figure 2. Mean isotherms and streamlines for the turbulent convection due to two parallel line sources (after Rouse, Baines and Humphreys 1953). F refers to the flux of heat from unit length of each source.

(d) *Maintained point source : turbulent flow : stratified ambient fluid*

The case in which the turbulent plume from a point source of heat rises through fluid which has a vertical gradient of density is of the greatest interest to meteorologists, although so far as I know no experiments of this kind have been made. If the surrounding atmosphere is unstably stratified, the convection plume will be accelerated in some way, whereas with stable stratification it seems likely that the temperature in the plume will at some stage fall below that of the atmosphere at the same level so that no further rise can occur. Despite these more complicated circumstances, it appears that methods of the kind already used are capable of producing definite results, at any rate for certain atmospheres.

It will first be necessary to assume the existence of a similarity solution, such that the velocity and temperature have similar distributions in all horizontal planes, their magnitudes having a power-law dependence on z; this is not a dangerous assumption in itself, since if such a solution does not exist we shall not find one! Thus if T_1 is the temperature of the surrounding atmosphere, and T_0 is the value of T at $z = 0$,*

$$\overline{w} = z^m \times \text{func}\,(r/R),$$
$$g\,\overline{(T - T_1)}/T_0 = z^n \times \text{func}\,(r/R),$$
$$R \propto z^l.$$

In addition, I shall assume that the horizontal inflow velocity at the side of the plume is proportional to the magnitude of the mean vertical velocity at the same level; the idea here is that the magnitude of the turbulent velocity fluctuations within the plume will be proportional to the mean vertical velocity, and the inflow velocity will in turn be proportional to the turbulent velocity. The inflow velocity is proportional to

$$\frac{1}{2\pi R}\frac{d}{dz}\int_0^\infty \overline{w}\, 2\pi r\, dr,$$

and if this is to be proportional to $\overline{w}$, we must have

$$l = 1,$$

showing that the plume is again conical.

The unknown powers m and n are now determined by the equations expressing the balance of momentum and heat. The increase of flux of vertical momentum with z is a consequence of the buoyancy force, i.e.

$$\frac{d}{dz}\int_0^\infty \overline{w}^2\, 2\pi r\, dr = \int_0^\infty g\,\overline{\left(\frac{T - T_1}{T_0}\right)}\, 2\pi r\, dr,$$

which requires

$$2m + 1 = n + 2.$$

The increase of flux of heat with z is due to the inflow at the side of the plume, i.e.

$$\frac{d}{dz}\int_0^\infty \overline{w}\,\overline{T}\, 2\pi r\, dr = T_1\frac{d}{dz}\int_0^\infty \overline{w}\, 2\pi r\, dr,$$

which can be written in the alternative form

$$\frac{d}{dz}\int_0^\infty \overline{w}\, g\,\overline{\left(\frac{T - T_1}{T_0}\right)}\, 2\pi r\, dr = -\frac{g}{T_0}\frac{dT_1}{dz}\int_0^\infty \overline{w}\, 2\pi r\, dr.$$

* If the density variations of the surrounding atmosphere are accompanied by appreciable variations in the absolute pressure, as will frequently be the case in meteorological contexts, it will of course be necessary to replace the excess absolute temperature $T - T_1$ in these equations by the excess potential temperature $\theta - \theta_1$, where θ_1 is the (non-uniform) potential temperature of the atmosphere.

This relation requires first that the surrounding atmosphere has a power-law lapse rate,

$$\frac{g}{T_0}\frac{dT_1}{dz} = -C z^p,$$

where C, so it will appear, must be positive and second that

$$m + n + 1 = p + m + 2.$$

Hence

$$m = \tfrac{1}{2}(p+2), \quad n = p + 1,$$

and the velocity and temperature distributions have the form

$$\overline{w} = (C z^p)^{1/2} \times z \times \text{func}\,(r/R),$$

$$g\,\overline{(T - T_1)}/T_0 = (C z^p) \quad \times z \times \text{func}\,(r/R),$$

$$R = \beta z,$$

where β is a constant.

The vertical flux of heat due to the plume is proportional to $z^{1/2\,(3p+8)}$, and the value at $z = 0$, which will be a measure of the strength of the source giving rise to the plume, will be zero, finite, or infinite, according as $p > -8/3$, $p = -8/3$ or $p < -8/3$. In the case $p = -8/3$, the above relations can only be satisfied with $C = 0$, and the whole analysis reduces to that given previously for a uniform atmosphere. Thus, discarding the case of a source of infinite strength as not being of practical interest, we are left with $p > -8/3$, and a zero value of the flux of heat at $z = 0$.

Although it has emerged that the similarity solution is restricted to cases in which the atmosphere is unstably stratified, it has produced some interesting results. It demonstrates analytically the almost obvious fact that the development of a turbulent heat plume in an unstable atmosphere does not need a heat source. The flux of heat at the apex of the cone containing the plume is zero, so that in practice any small disturbance would be sufficient to enable the plume to develop. Indeed the presence of the boundary is now irrelevant, and a rising plume could begin at any point in the unstable atmosphere if a suitable small disturbance occurs there. In practice, it is not likely that a conical plume with its apex at a point away from the boundary would persist, since it would be carried away and disrupted by casual winds, but a weak maintained source at the earth's boundary would give rise to a fairly persistent plume of the above kind.

Inasmuch as it will usually be possible to describe the variation of gradient of the ambient temperature by a power-law over a small range of height near the ground, solutions of this kind should have approximate validity for weak sources in all unstable atmospheres. As an example, we can put $p = -1$, which corresponds to an unstable layer whose temperature gradient decreases with height, just as it does in the atmosphere under conditions of strong radiation to the ground. The mean velocity here varies as $z^{1/2}$, the excess temperature in the plume is independent of z, and the heat flux varies as $z^{5/2}$.

Similarity solutions apparently do not exist for a stable atmosphere, and different methods of analysis are needed. G. I. Taylor (1946) has given an approximate method of analysis and has succeeded in predicting the height to which the turbulent plume from a maintained line source would rise in an atmosphere with a uniform positive temperature gradient.

(e) *Instantaneous point source : turbulent flow*

As an interesting extension of the type of problem which I have been describing, one may ask : what happens when a finite quantity of heat is suddenly released at a

point in the atmosphere? Clearly it will rise and will set up a turbulent motion, and cold air will be entrained into the rising cloud, just as in the case of the plume from the maintained source. If we are willing to admit the existence of similarity solutions such that the distributions of velocity and temperature relative to the moving centre of the cloud remain the same as the cloud rises – which I think is reasonable, although more arguable than the corresponding assumption for a maintained source – we can employ dimensional arguments to obtain a number of useful results. Again the essential effect of the source can be thought of as a release of weight-deficiency of amount $\rho_0 Q$, provided $(T - T_0)/T_0 \ll 1$, and Q is the sole parameter representing the initial conditions when the atmosphere is uniform.

If h is the height, at time t after the release of heat, of some convenient centre (say the mass-defect centre) of the rising cloud, and $\mathbf{r}$ denotes position relative to this moving centre, a similarity solution will be such that $\bar{w}$ and $\overline{(T - T_0)}/T_0$ – for a uniform atmosphere – are each equal to a function of $\mathbf{r}/R$, multiplied by some power of h, where R is some measure of the linear dimensions of the cloud. Dimensional arguments lead immediately to the forms:

$$\bar{w} = \frac{Q^{1/2}}{h} \times \text{func}\,(\mathbf{r}/R),$$

$$g\,\overline{(T - T_0)}/T_0 = \frac{Q}{h^3} \times \text{func}\,(\mathbf{r}/R),$$

$$R = \gamma h,$$

where γ is a constant. It follows also that h is proportional to $t^{1/2}$ and that the values of $\bar{w}$ and $\overline{(T - T_0)}/T_0$ at a fixed value of $\mathbf{r}/R$ are proportional to $t^{-1/2}$ and $t^{-3/2}$ respectively. The cloud grows by turbulent entrainment of surrounding fluid as it rises, and the effective radius of the cloud remains the same fraction of the height of its centre. So far as I know, no quantitative experiments related to this problem have been made. G. I. Taylor (1946) has obtained the above results, although not in exactly the same way.

It is also possible to find similarity solutions for the development of a rising cloud in the case of an unstable atmosphere with a power-law variation of temperature gradient, just as in the case of a plume from a maintained source. Again it turns out that the cloud can develop from a small disturbance in the fluid. The details are similar in type to those already given for the plume, and need not be reproduced here. These more-or-less spontaneous plumes and clouds seem to provide a definite mechanism for the vertical transfer of heat by convection in an unstable atmosphere over a fairly uniform boundary which cannot provide strong discrete sources.

(f) *Clouds and plumes appreciably lighter than the ambient fluid*

The whole of the foregoing analysis has been based on the assumption that $(T - T_0)/T_0$ and $(\rho_0 - \rho)/\rho_0$ are small compared with unity, and that as a consequence the only effect of the variation of density is to produce a buoyancy force on the lighter fluid. A density variation of only one per cent can produce significant convection currents in the atmosphere, whereas the same density variation has only a negligible effect on the inertial properties of the fluid. It will usually be the case that density differences occurring naturally in meteorology are small, and that the analysis already described is applicable, but I should like to consider briefly the qualitative changes that occur in the convection pattern when the density differences are not small.

The new feature that must now be allowed for is the appreciable difference between the inertia of the rising lighter fluid and that of the ambient fluid. To see the effect that

this will have when the lighter fluid is in the form of a cloud, released from some instantaneous source, let us trace the development of the motion, beginning with the early stage in which the motion is still laminar. If the cloud is roughly spherical, the buoyancy force will urge the whole cloud upwards, and the friction at the interface between the two fluids will restrain the outer layers of the spherical mass of lighter fluid. Thus a motion up the vertical axis and down the lines of longitude will develop in the sphere, rather like that shown in a photograph by Savic (1953) of the streamlines in a drop of water rising through castor oil. Vorticity, or shearing motion, is generated inside the cloud as a result of the friction at its outer surface and is distributed through the cloud by the motion up the vertical axis, whereas the fluid outside the cloud will be free from vorticity except in the thin boundary layer near the interface. Consequently turbulence will develop inside the cloud, when the Reynolds number is large – as it will be for meteorological cases. This turbulent motion inside the cloud will produce velocity fluctuations near the interface which will be able to penetrate into the heavier fluid and to disrupt the cloud *only if the inertia of the fluid within the cloud is not small compared with the inertia of the fluid outside the cloud.* In the extreme case $\rho/\rho_0 = 0$, as when air bubbles rise through water, the turbulence of the motion within the bubble can have no effect on the motion of the water; there is thus no turbulent entrainment, and the shape of the upper portion of the interface is smooth and fairly steady, and has a shape determined by considerations that do not involve the internal motion of the bubble.

At the extreme of small density difference, then, we have the situation analysed in the preceding section, with vigorous turbulent entrainment of the ambient heavier fluid, rapid dilution of the density difference and a decrease of the rate of rise of the cloud. At the extreme of large density difference there is no turbulent entrainment, very little dilution and very little decrease in the rate of rise. As the density ratio varies there will be a smooth transition between these extremes. Mr. D. B. Thomas has taken some photographs for me in order to illustrate this transition, and a typical set is shown in Fig. 3 (Plate V). The same volume of lighter fluid has been released in a cubical tank one foot deep in each of these photographs, and the values of ρ/ρ_0 shown refer to the density of the lighter fluid before release.

It should be noted that the air bubble in water is not quite the right limiting case, since air is not miscible with water. In the case of miscible fluids* some small amount of entrainment will occur by molecular diffusion across the interface, and there will also be a little turbulent entrainment at the rear of the cloud, however close to zero the density ratio may be. Thus there will be slow initial dilution, and as this raises the density of the cloud the turbulent entrainment will become more vigorous. An example of a cloud of very light fluid rising through ambient fluid with which it is miscible is provided by an atomic bomb explosion. The explosion creates a mass of very hot gas, so that the cloud begins into ascent with a density ratio close to zero. The rate of entrainment is initially very small, and gradually increases, producing the typical cauliflower appearance, rather like a compact cumulus cloud.

The fact that air and water are not miscible makes the rise of large air bubbles through water a simpler matter than it would otherwise be, and several interesting features of the motion have been revealed in an investigation by Davies and Taylor (1950).

3. Problems of free convection due to sources distributed over a surface

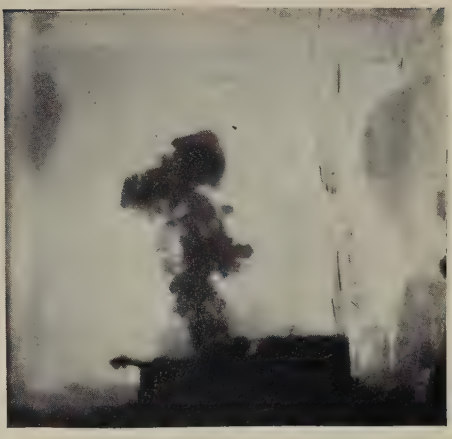
When a source of heat, or weight-deficiency, is distributed over a large surface, the resulting convective motion is quite different in many respects from that due to discrete

* The fluids used in the first four photographs of Fig. 3 were miscible.

(a) $\rho/\rho_0 = 0{\cdot}98$.

(b) $\rho/\rho_0 = 0{\cdot}83$.

(c) $\rho/\rho_0 = 0{\cdot}72$.



(d) $\rho/\rho_0 = 0{\cdot}63$.

(e) $\rho/\rho_0 = 0$ (air).

Figure 3. Photographs of the rising cloud resulting from the release of a small volume of fluid of density ρ in ambient fluid of density ρ_0.

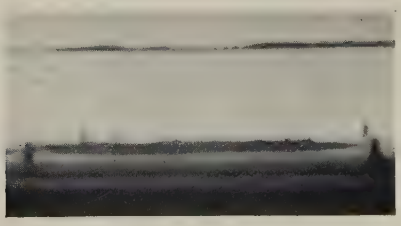


Figure 4. Laminar plumes in unstable fluid of finite depth, at Rayleigh number below that needed for thermal turbulence, and before cellular motion has developed (due to Thomas) (see p. 355).

sources. A discrete source sets up a convective motion whose general features can readily be guessed; for example, when the source is the only one present, the temperature and vertical velocity will have their largest values vertically above the source. In the case of a source distributed over a surface the pattern of the motion, being influenced by the presence of boundaries, is not so closely determined by the existence of the source alone, and cannot be analysed so readily. There is, moreover, the new feature that when the source is distributed uniformly over an effectively infinite horizontal surface, the fluid above the surface will be in static equilibrium when it is at rest, so that there will arise a problem of stability, requiring a determination of the conditions under which the equilibrium will be upset by a small disturbance. In general the conditions for instability do not differ significantly from the condition that the density should increase with height, but the determination of the exact criterion, taking account of molecular damping of disturbances, is an interesting problem in itself and the form of the disturbance that is found to grow most rapidly has a bearing on the kind of motion that occurs under strongly unstable conditions.

There is not sufficient time for me to describe the established work on the various cases of convection due to heated surfaces, or even to describe all the recent work, and I propose, therefore, to consider only cases in which the boundaries are horizontal planes, this being the class of problem which will have applications in meteorology. There will be two related cases, first, fluid between two horizontal planes at different temperatures, this being a valuable case for study since it offers simultaneously opportunities for relatively simple theory and controlled experiments, and second, semi-infinite fluid lying above a heated plane.

(a) *Two horizontal boundaries at different temperatures*

Under this heading you will undoubtedly expect me to mention the Rayleigh-Jeffreys criterion for instability and the formation of hexagonal Bénard cells. These cells have been described many times in meteorological literature, and have perhaps been given undue prominence by reason of their unassailable mathematical backing. The resemblance between cellular patterns found in a laboratory dish and certain regular cloud patterns has often been remarked, but I doubt if the connexion is always as close as is suggested. Cellular convection is usually a weak, sensitive motion, easily disturbed by cross-currents and lack of uniformity of the boundaries, and it will happen only rarely that a regular array of cells can develop and retain an orderly appearance in the disturbed conditions prevailing in the atmosphere. There is also the point that when the depth of the unstable layer is large, the temperature differences needed for the cellular pattern to break down to a turbulent motion – which would tend to destroy the visible regularity – are extremely small. For instance, an unstable layer of air 10 m thick between two free surfaces would develop a steady cellular convection only if the temperature difference between the surfaces is greater than about 10^{-8} °C and *less* than about 10^{-6} °C. Of course, if an eddy diffusivity due to some background turbulence in the atmosphere replaces the molecular diffusion, the required range of temperature difference is much larger but it is then necessary to explain why the same background turbulence should not break up the regular arrangement of cells.

Taking for granted, then, the conventional Rayleigh-Jeffreys analysis of the stability of fluid with a linear variation of temperature between two boundaries, and the usual experimental verification, I can pass on to other aspects of the problem. A useful extension of the analysis would be to cases in which the density does not vary linearly, and in particular to a case in which the density increases rapidly with height near the lower surface and flattens out to some uniform value at greater heights. This distribution is like that

obtained when the lower surface is heated rapidly and before the heat has had time to be spread by conduction throughout the fluid between the two boundaries. A stability problem closely analogous to this has already been worked out by Görtler (1940). The analogy between the stability of stratified fluid between two horizontal planes and the stability of steady viscous flow between rotating concentric circular cylinders, the force of gravity being replaced by centrifugal force, has been noted in the literature, and the rotating-fluid analogy of the situation just described is the flow of a uniform stream, with a boundary layer, along a concave wall. Görtler has found that this flow is unstable to a disturbance in the form of regularly-spaced vortices in the boundary layer with axes parallel to the flow and that the velocity profile is relevant chiefly through its influence on the displacement thickness of the boundary layer. The analogy between stratified fluid and rotating flow is not exact, and details of the stability of the stratified fluid have not been worked out, but I think it is fairly certain that the results for the two problems would be similar, and that the stratified fluid would behave approximately as though it were bounded by two planes distance h apart, with a linear variation of density between the planes, where h is proportional to

$$\int_0^\infty (T - T_0)/T_0 \, dz \quad (T_0 = \text{uniform temperature at } z = \infty),$$

i.e. h is proportional to the total quantity of heat supplied to the fluid through the lower surface. A corollary of this result is that as heat is supplied through the lower surface, with a fixed value of the temperature of that surface, the effective Rayleigh number increases steadily and the fluid becomes steadily less stable. So far as disturbances of the conventional kind are concerned, the least stable state should be the final one, in which the entire temperature variation between the two boundaries is linear, and if this state is not unstable none of the earlier states will be unstable either. Thus rapid heating of the lower boundary should not matter, provided the total quantity of heat supplied is not more than that needed for instability when the temperature variation between the two boundaries is linear.

I have remarked on this extension of the conventional analysis partly because a boundary-layer type of temperature profile occurs often in practice and partly because of its relevance to an intriguing phenomenon observed first by Chandra (1938) and later by Dassanayake (unpublished thesis, Imperial College) and by de Graaf and van der Held (1953). Chandra found that when two rigid conducting boundaries are close together – less than about 1 cm apart, in the case of air – a seemingly new kind of disturbance is generated at Rayleigh numbers considerably less than the critical value worked out by Jeffreys (1928). This new disturbance seems to consist of a number of columns, not regularly spaced and not fixed in position, within which there is an upward motion from one plate to the other. Recently Sutton (1950) has stimulated interest in this phenomenon and has pointed out the remarkable fact that the empirical condition for the occurrence of this columnar mode of convection seems to be that $(T_1 - T_0)/T_0$ must be greater than about 0·025, where T_0 and T_1 are the temperatures of the boundaries. This criterion for the columnar type of instability is remarkable because it depends neither on the distance between the boundaries, nor on the properties of the fluid. If it is a genuine criterion for instability, it is very difficult to see why it does not depend in some way on the molecular diffusion parameters. Moreover it is difficult to explain the occurrence of instability at Rayleigh numbers below the accepted critical value. It is true that the conventional analysis assumes a type of disturbance that is periodic in the horizontal plane, but if instead we assume, for example, that the disturbance is symmetrical about a vertical axis, the corresponding critical Rayleigh number can be shown to be even

larger. Sutton has put forward the view that the conventional assumption of a linear variation of temperature between the boundaries may be at fault, and that a rapid fall in temperature near the lower boundary may give rise to a different stability limit, but in view of the analogy with rotating flow, explained above, I should have expected this boundary-layer type of temperature distribution to be even more stable than a linear variation with the same total temperature difference. Putting the matter crudely, a rearrangement of the temperature distribution so that the whole of the temperature difference occurs in a portion only of the layer gives a sub-layer with a Rayleigh number smaller than that for the original distribution, since $A \propto (T_1 - T_0)\, h^3$.

When the difference between the temperatures of the two boundaries is sufficiently large, ordered cellular motion gives place to a turbulent convection. This case of thermal turbulence between two horizontal planes, involving no mean shearing motion, and having its origin entirely in buoyancy forces, is of considerable interest from the meteorological point of view, and it is also a simple, well-defined, fundamental case, well suited for study by the theoretician. Experimental studies (Jakob 1949, de Graaf and van der Held 1953), have suggested that the transition from a laminar cellular convection to a disordered turbulent convection occurs at a Rayleigh number somewhere near 50,000, and that the relation between heat transfer and Rayleigh number changes at this point. A very comprehensive and careful set of experiments has recently been carried out by Malkus (1954a) and has yielded some interesting and unexpected results. If these experiments and Malkus's interpretation of them are correct, it seems that our ideas about thermal turbulence will need to be different from those about turbulence generated by a shearing motion.

Malkus used both distilled water and acetone as the fluid between the parallel boundary plates, and was able to measure the heat transfer between the plates up to Rayleigh numbers of about 10^{10}. The results for water, taking the Rayleigh number up to 10^7, have the puzzling feature that the heat transfer varies linearly with Rayleigh number between a number of transition points at which the slope changes abruptly. Malkus points out that the transitions occur at Rayleigh numbers which are close to those that are critical for the growth of disturbances filling sub-multiples of the layer of fluid. For example, the third transition occurs at a Rayleigh number of about 55,000, which is also approximately the critical Rayleigh number for the growth of cellular disturbances which would fill one-third of the layer. These higher modes of disturbance are well understood as a part of the mathematical theory but one would not have expected them to be realized in practice, because the first-order mode – i.e., the ordinary Bénard cell – always appears first and produces conditions different from those contemplated in the theory. At a Rayleigh number of 55,000, the first-order mode of disturbance is present in such a vigorous form that the fluid is far from being at rest and the temperature variation departs considerably from the assumed linear variation.

When the heat transfer coefficient is plotted as a function of Rayleigh number over the whole range of Rayleigh numbers, on the one diagram, Malkus finds that the various transitions merge into a smooth curve, except for one at a Rayleigh number of about 10^6. At this Rayleigh number the behaviour of heat transfer coefficient changes; from being proportional to $A^{1/4}$ approximately, it becomes proportional to $A^{1/3}$ approximately, and Malkus records that there appears to be a marked increase in the intensity and persistence of the horizontal motions of the fluid. Power-law variations of the heat transfer coefficient, with these same powers $\frac{1}{4}$ and $\frac{1}{3}$, are well-known in the subject of heat transfer and are usually found to occur when the thermal and velocity boundary layer, close to the heated surface is laminar or turbulent respectively. For instance, when a heated flat plate is held vertically, the heat transfer coefficient varies as $A^{1/4}$, corresponding to a laminar boundary

layer at the plate, for values of A (based on the length of the plate) up to about 10^9, whereas for larger values of A the boundary layer is found to be turbulent and the heat transfer coefficient varies as $A^{1/3}$. In our case of fluid between parallel planes there is no mean velocity parallel to the boundaries so that the notion of a velocity boundary layer is not immediately clear. Perhaps the situation is that for values of A above about 50,000 (the value at which simple cellular motion of the Bénard type breaks down) but less than 10^6, a number of complicated cellular modes of disturbance can grow, but no steady motion is possible, so that the motion passes through an ever-changing sequence of patterns. The distinction between this and a genuine turbulent motion occurring for values of A above 10^6 is not clear, but it may be that when A, which is effectively a Reynolds number for the convective motion, is large enough the *individual* convection currents rising from the lower boundary can become turbulent.

Malkus (1954b) has devised an interesting theory of the heat transport of thermal turbulence, involving the idea that the convective motion adjusts itself so as to make the heat transport a maximum, consistent with those forms of cellular motion that can exist at that Rayleigh number; if his theory is right, it may open up a number of new developments in other branches of turbulence research.

(b) *Semi-infinite fluid above a heated horizontal boundary*

I suppose that, of all the different cases of free convection, the motion due to a single horizontal infinite heated boundary is the case that has most interest for the meteorologist. It is also one of the most difficult to think about, since it is not clear that the problem is specified completely when we say simply that the temperature of the fluid is uniform at large distances from the boundary. In the simple case of pure conduction, without either cellular or turbulent convection, the situation is clear enough; heat flows from the boundary towards the fluid at infinity, and the mean temperature everywhere in the fluid rises continually, no steady state being reached in a finite time. Under certain conditions the region of fluid near the boundary, where the temperature gradient is largest, is unstable and a cellular convective motion is set up, as already explained. When sufficient heat has been transferred across the lower boundary, a disordered convective motion is set up, and with still more heating ' thermal turbulence,' is generated : can there be a quasi-steady state under these conditions ? The temperature at large distances from the boundary must be rising, in order to absorb the heat transferred across it, but there exists the possibility of a quasi-steady state such that the distribution of mean temperature near the boundary is steady, this part of the fluid acting merely as a conveyor of the heat from the boundary to the fluid at some distance away. It is important that this question of the existence of a quasi-steady state should be resolved, because the use of dimensional analysis, based on assumptions of similarity solutions, depends critically on the right choice of the physical quantities that determine the velocity and temperature distributions uniquely. Most of the discussions of the problem have assumed implicitly the existence of this quasi-steady state, and although I think they may well be correct, the point does need further thought.

The available observations of the distribution of temperature in the fluid above a heated surface at Rayleigh numbers well above that needed for cellular motion to occur – most of them made in the atmosphere, and some in a laboratory by Ramdas and Malurkar (1932) - all emphasize the fact that the magnitude of the gradient of temperature is greatest near the surface and decreases rapidly with height. Ramdas and Malurkar have revealed the general structure of the motion above a heated metal plate of $1\frac{1}{2}$ ft $\times$ $4\frac{1}{2}$ ft by the simple device of making the flow visible with steam (see their Plate 2). They also record

that in a similar experiment hot air near the plate (distinguished by being dark and free from smoke and dust, when the remainder of the fluid contains visible smoke – this is a molecular effect, the large smoke particles being forced down a thermal gradient and away from the plate) shot up in several tongues of ascending air, their width near the hot surface being $1\frac{1}{2}$ to $2\frac{1}{2}$ cm broad and decreasing to an apex at a height of several centimetres. This decrease of width with height I take to be part of the process of drawing-in which occurs near an extended source and of which the double line source has provided an example. They go on to say that the hot columns were not stationary, but developed and moved about in a random manner with a tendency to drift with a general wind, if any, across the plate. In his work on convection between two horizontal plates, Malkus also noted visually the existence of fairly persistent columns of ascending air. I think that these ascending hot columns are probably a basic feature of all kinds of convective motion in an unstably stratified fluid. They even occur between two parallel boundaries at Rayleigh numbers below that needed for thermal turbulence before cellular motion has developed, as can be seen in a photograph by Thomas reproduced as Fig. 4 (Plate V); these laminar columns rise slowly, and turn over, sometimes before they meet the upper surface, and in time become organized into the usual hexagonal cell.

I think it is probable that the ascending hot columns in the case of turbulent convection above a single boundary are similar to the rising plumes considered in Section 2 (d). The existence of discrete plumes above a nearly uniform surface is explained by the fact that when the ambient fluid is unstable, the plumes do not need a finite source of heat and will develop from a small disturbance, the flux of excess heat due to the plumes increasing with height. Once formed, these plumes would be self-maintaining, but, having no anchor on the boundary, they would drift about and develop under the influence of casual currents. Such plumes produce a very small flux of excess heat across planes close to the boundary and in this region molecular conduction must be responsible for most of the heat-transfer; the motion at the bottom of the plumes must also be laminar for a short distance, before developing into a turbulent flow with entrainment.

It is difficult to give an analytical account of the whole problem, taking into account the region close to the boundary where molecular conduction becomes important, but something can be said about the region where the plumes or columns have become properly turbulent. Of course, if we exclude the region near the boundary, we cannot make use of the boundary condition giving the temperature at zero height. In this respect the problem is like that of determining the mean velocity in turbulent flow over an aerodynamically smooth wall. If we make the assumption that the mean temperature of the fluid in the turbulent region is *steady*, and is determined uniquely by the vertical heat flux per unit area, the distribution of mean temperature can be determined by dimensional arguments, as Priestley (1954) has recently pointed out. There must first be the assumption of a similarity solution such that the mean temperature varies as some power of z, the position of the origin of z being left undefined in view of the existence of the conduction layer although it will certainly lie close to the boundary. Then if $\rho_0 f$ is the mean flux of weight-deficiency through unit area of the horizontal boundary, dimensional requirements lead to

$$g\,\overline{(T - T_0)}/T_0 \propto \frac{f^{2/3}}{z^{1/3}}.$$

From the form of this relation, T_0 must be the mean temperature far away from the boundary, so that it is possible to do without information about the temperature of the heated boundary. It can be shown quite readily that this is also the variation of temperature with height that would be obtained if there were spontaneous conical turbulent plumes distributed over the horizontal boundary in an atmosphere whose lapse rate is

proportional to z^p, p being chosen to make the vertical heat flux independent of z. If a steady state is *not* imposed, the heat flux can vary with z and the value of p can then be quite general. Priestley has examined all the available meteorological data about the temperature distribution within a few metres of the earth's surface, and claims that there is good agreement with the above relation.

(c) *Convection in the presence of a forced wind*

As a postscript I should like to refer back to the restriction I laid down at the beginning, namely, that I should consider only problems of free convection. It will not happen often in the atmosphere that convection occurs in the absence of winds from some other source. These winds may be gentle, but so too are convection currents usually, so that it is very desirable to know something of the way in which convection and buoyancy effects interact with a forced wind. I do not propose to consider this question, since it is complicated and not well-understood, but I should like to report that a programme of study is in progress at Cambridge. Nicholl has constructed equipment that will allow an investigation of the convection due to a heated – or cooled – region of a horizontal boundary across which a wind is blowing, the general arrangement being as in Fig. 5.

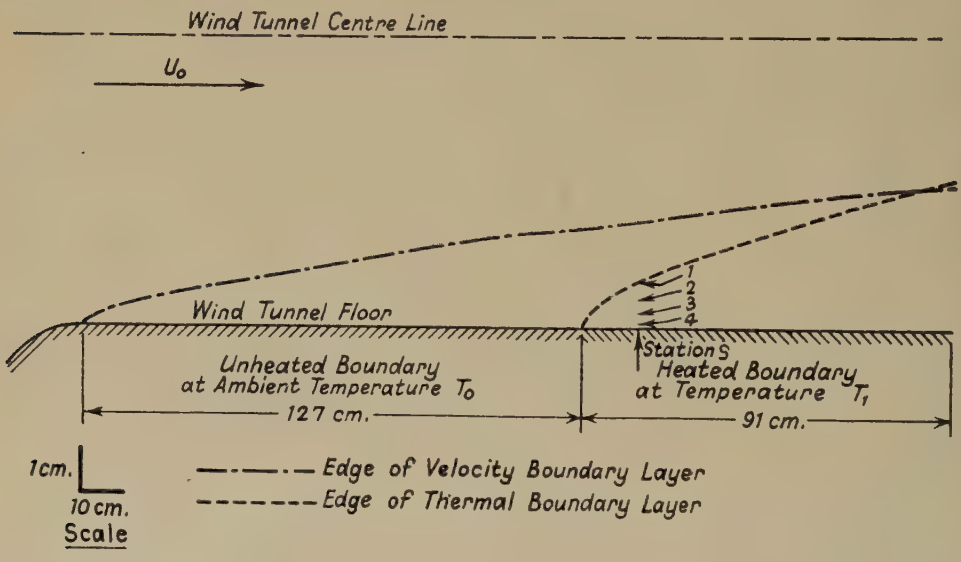


Figure 5. Wind-tunnel arrangement for investigation of effect of a heated boundary on a turbulent boundary layer (unstable case).

The object of the investigation is to see how the heating or cooling at the plate embedded in the boundary modifies the flow in the turbulent boundary layer passing over it. With a sufficiently low temperature of the plate it may be expected that the stable stratification will suppress the turbulent fluctuations of velocity and will ultimately cause it to become laminar, as sometimes happens at the earth's surface on clear nights with strong outgoing radiation. So far Nicholl's experiments have been confined to the case of a hot lower boundary, and the effect here is to make the turbulent velocity fluctuations more vigorous. There are also interesting connexions between the fluctuations in velocity and temperature, of which Fig. 6 shows a sample. At position 1 near the outer edge of the thermal boundary layer, fluctuations in temperature appear only intermittently. Even at position 2, there are periods of thermal uniformity, corresponding to occasions when the vertical velocity is towards the wall and carries unheated air to the measuring

instrument. At position 3, the strong correlation between low u and high T' is evident, both of these properties being associated with a rising fluid element. There is also a marked skewness of the temperature fluctuations, large positive fluctuations in temperature being much more probable than large negative fluctuations. Position 4 is very close to the boundary, and the reduction in correlation between u and T' and the decrease in the skewness of T' are presumably due to the fact that transfer by conduction is becoming important here. A measure of the overall effect of the buoyancy forces of the flow is given by the observation that the root-mean-square of the velocity fluctuation parallel to the stream increases by as much as a factor 1·7 due to the heating even though the Richardson number is relatively small; the mean velocity parallel to the boundary also increases, just as it does when a wind blows across a line source of heat.

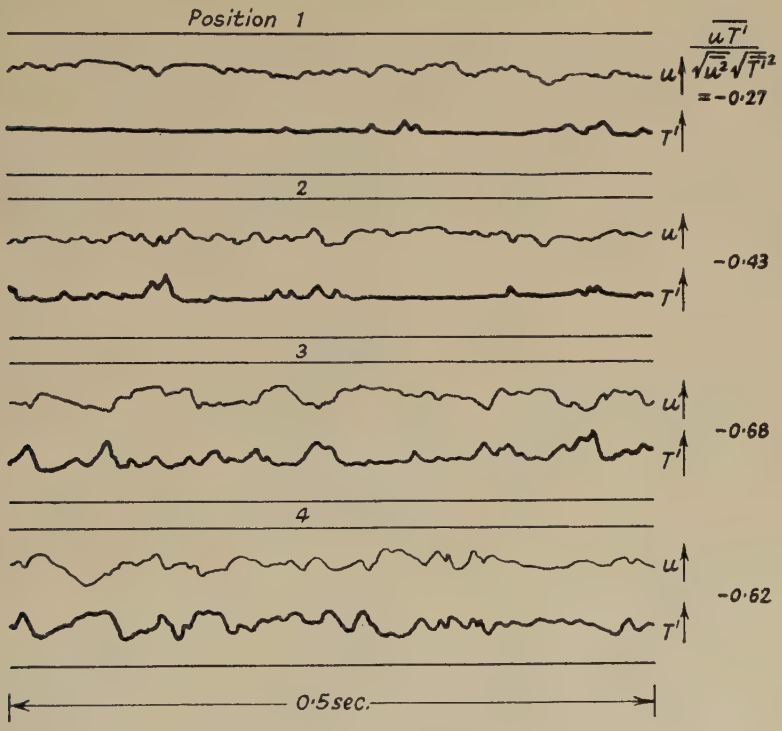


Figure 6. Simultaneous velocity and temperature fluctuations at station S (due to Nicholl). $U_0 = 240$ cm/sec, $T_1 - T_0 = 80°C$, thermal boundary layer 1 cm thick.

That concludes my very hasty survey of some problems of free convection that seem likely to be of interest in meteorology. All of them have their difficulties, but I hope to have shown that dimensional arguments, based on the assumption of a similarity solution, can often provide a surprisingly large part of the information required. Also, as I remarked before, there seems to be considerable scope for experimental work under controlled conditions in the laboratory.

I am conscious of the fact that, for lack of time, as well as lack of ability, I have not indicated the meteorological significance of many of these problems of free convection, nor have I related them to the extensive research on convection done from a meteorological point of view. That would be an interesting and important task, and my neglect of it does not imply that I am unaware of the need for it. I leave it to be undertaken by some more qualified person.

Acknowledgments

It is a pleasure to thank Dr. T. H. Ellison, Mr. B. R. Morton, Mr. C. I. H. Nicholl and Mr. D. B. Thomas for their willing and invaluable help in the preparation of this lecture, and Sir Geoffrey Taylor for making available to me some very relevant unpublished papers on convection problems.

References

Chandra, K. 1938 'Instability of fluids heated from below,' *Proc. Roy. Soc.* A, **164**, p. 231.

Davies, R. M. and Taylor, G. I. 1950 'The mechanics of large bubbles rising through extended liquids and through liquids in tubes,' *Ibid.*, **200**, p. 375.

Görtler, H. 1940 'On the influence of wall curvature on the production of turbulence,' *Z. angew. Math. Mech.*, Berlin, **20**, p. 138.

de Graaf, J. G. A. and van der Held, E. F. M. 1953 'The relation between heat transfer and the convection phenomena in enclosed plane air layers,' *Appl. Sci. Res.*, A **3**, p. 393.

Jakob, M. 1949 *Heat Transfer*, **1**, New York (Wiley).

Jeffreys, H. 1928 'Some cases of instability in fluid motion,' *Proc. Roy. Soc.* A, **118**, p. 195.

Malkus, W. V. R. 1954a 'Discrete transitions in turbulent convection, *Proc. Roy. Soc.* A (in the press).

1954b 'The heat transport and spectrum of thermal turbulence,' *Ibid.* (in the press).

Priestley, C. H. B. 1954 'Convection from a large horizontal surface,' *Austr. J. Phys.* **7**, p. 176.

Railston, W. 1954 'The temperature decay law of a naturally convected air stream,' *Proc. Phys. Soc.* B, **67**, p. 42.

Ramdas, L. A. 1953 'Convective phenomena near a heated surface,' *Proc. Ind. Acad. Sci.*, **37**, p. 304.

Ramdas, L. A. and Malurkar, S. L. 1932 'Surface convection and variation of temperature near a hot surface,' *Ind. J. Phys.*, **7**, p. 1.

Rankine, A. O. 1950 'Experimental studies in thermal convection,' *Proc. Phys. Soc.* B, **63**, p. 225.

Rouse, H. 1947 'Gravitational diffusion from a boundary source in two-dimensional flow,' *J. Appl. Mech.*, **14**, A, p. 225.

Rouse, H., Baines, W. D. and Humphreys, H. W. 1953 'Free convection over parallel sources of heat,' *Proc. Phys. Soc.* B, **66**, p. 393.

Rouse, H., Yih, C. S. and Humphreys, H. W. 1952 'Gravitational convection from a boundary source,' *Tellus*, **4**, p. 201.

Savic, P. 1953 'Circulation and distortion of liquid drops falling through a viscous medium,' *Nat. Res. Coun., Canada Rep.*, No. MT-22.

Schmidt, W. 1941 'Turbulent propagation of a stream of heated air,' *Z. angew. Math. Mech.*, Berlin, **21**, pp. 265 and 351.

Sutton, O. G. 1950 'On the stability of fluid heated from below,' *Proc. Roy. Soc.* A, **204**, p. 297.

Taylor, G. I. 1946 'Dynamics of a mass of hot gas rising in air,' U.S. Atomic Energy Commission, MDDC-919.

Yih, C. S. 1951 'Free convection due to a point source of heat,' *Proc. First U.S. Nat. Cong. Appl. Mech.*, p. 941.

1952 'Laminar free convection due to a line source of heat,' *Trans. Amer. Geophys. Un.*, **33**, p. 669.

551.551

The partition and intensity of eddy energy at the 91-metre level during unstable conditions as observed at Brookhaven National Laboratory

By ROBERT A. McCORMICK
U.S. Weather Bureau Office, Upton, N.Y.

(Communicated by H. Charnock; manuscript received 29 October 1953, in revised form 1 March 1954)

Summary

The magnitude and partition of eddy energy is reported for a series of observations near 91 m in which the period of averaging varied from 10 sec to 57 min. Of the 15 observations studied, 14 were made during unstable conditions typical of the predominant daytime regime found at Brookhaven in the lower 125 m. By means of moving averages as frequency filters, three sections of the eddy-energy spectrum are investigated. It was found that (a) the magnitude of the turbulent energy is proportional to the rate of energy supplied by the Reynolds's stresses and buoyancy forces, (b) the partition of eddy energy becomes increasingly unequal with increasing eddy size, and (c) two turbulence regimes apparently exist at 91 m with characteristics which depend on the trajectory of the air.

1. Introduction

The kinetic energy of atmospheric eddies and its partition among the three orthogonal directions, are of fundamental interest in the study of atmospheric motions. In the adaptation of statistical theories of turbulence to problems of atmospheric diffusion, for example, an evaluation of the partition of eddying energy must be provided or assumed. It is the purpose of this paper to report on some measurements of those quantities, within a limited range of stability conditions, at about 91 m on the 125-m tower at the Brookhaven National Laboratory, Upton, Long Island, N.Y. The scales of measurement defined by the periods of averaging varied systematically from 10 sec to nearly one hour.

G. I. Taylor (1927), Scrase (1930), Best (1935), and others have made similar measurements with about the same time-scale, but most of their observations have been confined to elevations less than about 20 m and few have included the simultaneous measurement of all three components of turbulence. The general conclusion, based largely on observations made during neutral conditions, has been that the partition of eddy energy is not equal in the first few feet, but tends toward uniformity with increasing elevation (Sutton 1953). Recognizing the important restriction on the vertical turbulence spectrum imposed by the proximity of the ground, R. J. Taylor (1952) and Panofsky and Singer (1951), for example, have pointed out that isotropy may be found only in atmospheric eddies whose dimensions do not exceed the height of observation. This concept can have particularly important bearing on the choice of the ratio of diffusion coefficients utilized in problems of atmospheric dispersion, particularly from tall stacks, in which the scales of interest may extend well out of the inertial sub-range of the eddy spectrum. To carry the effects of ground suppression one step further, it seems reasonable to believe that with stability conditions constant, or narrowly limited in range, at a fixed height any tendency away from equipartition of the eddy-energy distribution should increase with eddy size. This hypothesis has been investigated by comparing the distribution of energy in three sections of the turbulence spectrum, and some information has been gained of the values of accompanying meteorological parameters.

2. Source of data

The wind speed was measured by a standard Bendix-Friez Aerovane; vertical deflection and horizontal direction were measured by the annular-fin bivane described by Mazzarella (1952a). The free periods and damping constants of the instruments as determined by Mazzarella (1952a, b) are :

Instrument	Damping constant (sec^{-1})	Free period (sec)
Aerovane	$\cdot 138\, u$	$12 \cdot 28/u$
Bivane		
vertical	$\cdot 112\, u$	$10 \cdot 06/u$
horizontal	$\cdot 141\, u$	$10 \cdot 06/u$

where u is the wind speed in m sec^{-1}. Both instruments were mounted on a boom extending from the south-west corner of the 125-m tower about 5 m and 6 m, respectively, from the open steel framework. Data for 15 one-hour periods, or ' runs ' were analysed; the wind transmitters were located at the 91-m level in 13 cases, and at the 108-m level in the other two. The vertical shear of the wind was determined from a separate Aerovane system which records the wind movement at 125 m and the difference in wind movement between that level and 46 m and 11 m. A system of resistance thermometers (Leeds and Northrup ' Thermohms ') at the 11-m, 46-m, and 125-m levels provided measurements of the vertical temperature gradients. During the course of the runs, the rate of travel of the charts of the Esterline-Angus recorders was about 1·27 cm per 10 sec, a time interval automatically recorded on the edge of the chart and adopted as the time over which mean values of vertical and horizontal angle and wind speed were read. Use of a smaller averaging period, except during high wind speeds, was not advisable in the light of the response properties of the instruments. The response characteristics of Aerovane rotors to fluctuating wind speeds have not been established. However, if it is assumed that they are similar to those of the vane anemometer, the investigations of Ower (1949) would indicate that the lag error in 10-sec averages would not be appreciable. In computing the orthogonal velocity components, it was assumed that the speed indicated by the Aerovane represented the true wind speed in the horizontal plane.

TABLE 1. Average conditions during observations

Case	Trajectory	$T_{125} - T_{11}$ (°C)*	$u_{125} - u_{11}$ (m/sec)	u_{91} (m/sec)	Intensity of turbulence g_x	g_y	g_z	τ (dynes/cm²)	K_m (10^5 cm²/sec)
1	land	− 1·4	2·6	5·5	0·19	0·17	0·15	5·16	4·86
2	water	− 1·2	2·4	6·6	0·12	0·09	0·06	1·80	1·82
3	land	− 1·6	3·6	9·0	0·15	0·12	0·10	9·13	9·69
4	land	− 1·7	5·5	12·9	0·15	0·13	0·07	13·91	14·90
5	land	− 1·5	6·3	13·1	0·16	0·13	0·07	15·88	10·13
6	water	− 1·4	1·8	7·0	0·12	0·17	0·12	3·19	3·85
7	water	− 1·3	1·4	4·5	0·15	0·13	0·08	0·88	1·62
8	water	− 1·9	2·2	6·6	0·11	0·11	0·11	3·83	11·63
9	land	− 2·0	3·4	8·0	0·17	0·14	0·11	6·84	19·75
10	land	− 1·3	3·0	6·0	0·21	0·15	0·12	6·34	4·28
11	water	− 1·3	4·7	10·3	0·13	0·09	0·07	4·68	3·68
12	water	− 0·8	8·4	15·3	0·13	0·07	0·06	9·50	1·77
13	land	− 2·0	3·7	8·0	0·16	0·15	0·10	5·88	18·15
14	water	− 1·5	7·1	13·2	0·17	0·12	0·07	7·23	6·86
15	water	− 1·7	2·7	5·9	0·15	0·14	0·13	3·23	7·15

* − 1·1°C for adiabatic lapse rate.

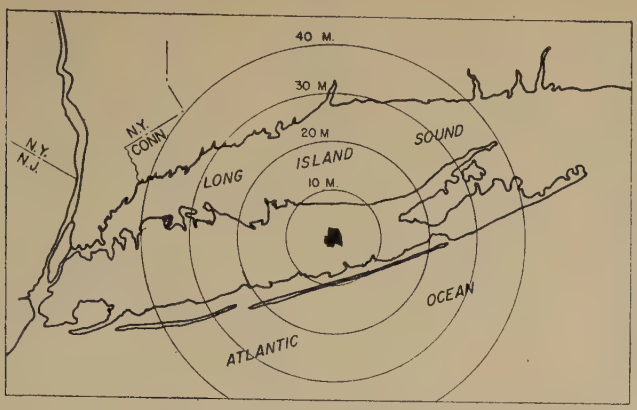


Figure 1. General location of Brookhaven National Laboratory.

3. The observations

The average conditions which prevailed during the hour of each run are given in Table 1. In the third column is tabulated the temperature difference in °C between the 125-m and 11-m levels. Then follows the velocity difference between those levels as indicated by the gradient Aerovane system, and in the fifth column is listed the mean velocity in the direction of the mean wind at 91 m. All runs were made during the daytime and, with only one exception, when the lapse-rate in the lower 125 m was unstable. In those cases the lapse-rate ranged from 110 to 180 per cent of the adiabatic and the wind-speed range was 4·5 to 13·2 m/sec. In the one stable run, the lapse-rate was about 73 per cent of adiabatic and the wind speed exceeded 15 m/sec. The unstable runs were made during turbulence regimes classified as 'B_1' gustiness at 125 m, as defined by Singer and Smith (1953), a regime which they found to have a percentage frequency of 42 per cent at Brookhaven, heavily weighted toward the daylight hours. Hence, the runs are samples of the predominant daytime conditions at the point of observation. The Brookhaven tower is located near the centre of Long Island, as shown by Fig. 1, in generally flat or slightly rolling countryside, which is wooded by scrub pine and scrub oak, some 6 to 8 m in height, interspaced with cleared areas and farming lands.

4. Analysis of total eddy energy

Under the assumption that the basic 10-sec mean wind components may represent 'instantaneous' discrete measurements, each can be expressed by

$$u_i = \bar{u}_i + u_i' \quad (i = 1, 2, 3) \qquad . \qquad . \qquad . \qquad (1)$$

where $\bar{u}_i$ is the over-all mean (over a period of 57 min) and u_i' the deviation of u_i from $\bar{u}_i$. In some cases, a trend existed in one or more of the components and the u_i were defined in terms of a line of regression,

$$u_i = (\bar{u}_i + b_i\, t) + u_i' \qquad . \qquad . \qquad . \qquad . \qquad (2)$$

where b_i was the slope of the line of regression, computed from the 10-sec mean data, and t, the time, was taken equal to zero at the middle of the observation. The mean-square deviations, i.e. the $\overline{u_i'^2}$, were computed for each run; they represent double kinetic energy per unit mass, but hereafter will be called simply the eddy energy.

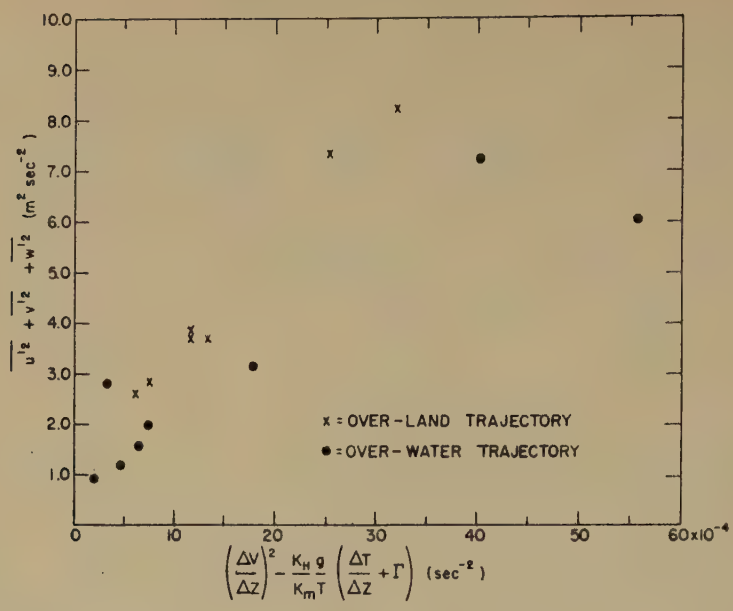


Figure 2. Variation of total eddy energy with energy supply, $K_H/K_m = 1$.

The ' gustiness ' classifications of the turbulence remained substantially unchanged during each run, and it seems reasonable to assume that the observed eddy energies existed during steady-state conditions. This being the case, the eddy energy should then be correlated with either the rate of supply of energy to the motions or with the dissipation of eddy energy into other forms. The first of these alternatives was investigated and in Fig. 2 the sums of the component energies, i.e., $\overline{u'^2} + \overline{v'^2} + \overline{w'^2}$, have been plotted against L. F. Richardson's expression for the rate of increase of the mean turbulent kinetic energy per unit mass evaluated for the layer between 11 m and 125 m assuming linear profiles of temperature and velocity. The first term is the square of the shear of the horizontal wind between those elevations (direction changes with height were small and neglected), and is proportional to the energy derived from the mean motion by the Reynolds's stresses. The energy expanded against gravity is represented by the second term where g is the acceleration of gravity, T the mean absolute temperature of the layer, $\Delta T/\Delta z$ the temperature gradient between 11 m and 125 m, Γ the dry adiabatic lapse rate, and K_H/K_m the ratio of the eddy conductivity to eddy viscosity. Mean values of Richardson's expression, called here the ' energy supply,' were computed for each run assuming K_H/K_m was unity.

It will be noted that, with one exception, the data divide into two categories, depending on the previous history of the air sampled. The solid circles are those cases in which the air had approached the tower from the south-east to south-west quadrant; the crosses represent cases with flow from the south-west to north sector. To carry the implications of this classification still further, it is assumed that the categories represent over-water and over-land trajectories, respectively (no runs were made when the wind was from the north-east quadrant since the instruments would be downwind of the tower structure and not exposed in ' free ' air flow). However, by reference to Fig. 1 it is evident that the trajectories are not as clear-cut or invariant as the classifications would imply. In the over-water cases it seems fairly certain that before traversing the 8 mi or so of land surface between the ocean and the tower, the air had been over water for at least the previous 18 to 24 hr. In 5 of the 7 over-land cases, while the last 12 mi or more of the

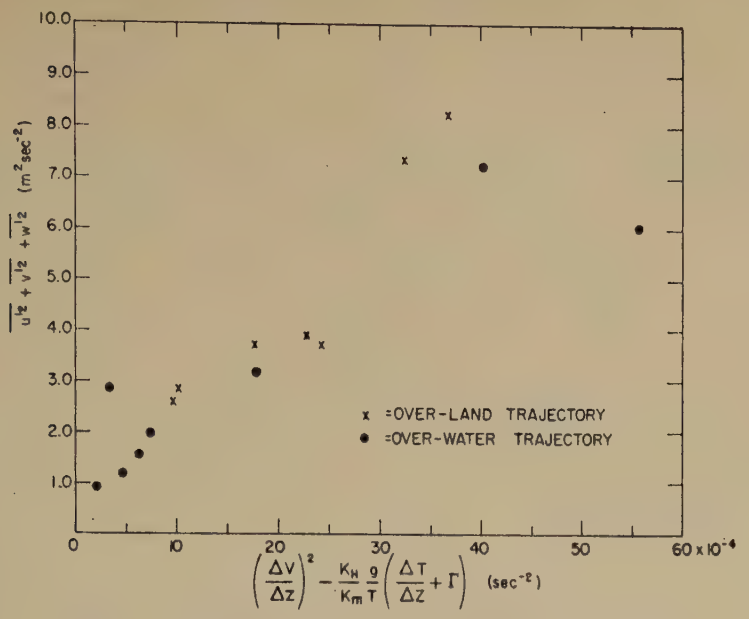


Figure 3. Variation of total eddy energy with energy supply. Over-land trajectory, $K_H/K_m = 5$; over-water trajectory, $K_H/K_m = 1$.

trajectories had been over Long Island, the previous 15 to 20 mi of travel had been over the Sound. Apparently the energy-supply term, as evaluated here at the tower, is not equally representative of the existing eddy energy at 91 m for all trajectories.

The observation at the extreme right of Fig. 2 is that of the stable run previously mentioned. During stable conditions it is commonly observed that turbulent motions are suppressed throughout large sections of the intermediate eddy spectrum where the period-range may be from a few to many tens of minutes. Presumably, any excess of energy extracted from the mean motion over that expended against gravity, in intermediate scales, is converted into turbulent energy in greatest measure by the small-scale eddies. If such is the case, one might expect a better fit for the stable observation if the eddy-energy analysis had included contributions from smaller eddies than those represented in the figure. Panofsky has made such an analysis and has kindly made the result available. His smallest period of averaging was 3 1/3 sec, instead of 10 sec, and he obtained a total eddy-energy value of 11·5 m^2 sec^{-2}. Although it has not been plotted, it is evident that this value fits the trend much better than the 6·0 m^2 sec^{-2} shown. This leads to the further speculation that perhaps in the over-water cases a higher percentage of total eddy energy is located at frequencies greater than those studied than is the case for over-land trajectories. However, the reason why this should be so is not clear in view of the distances over land which air with any trajectory must traverse to reach the tower.

Another feature of interest in the figure is that if the trends are extrapolated to zero energy supply, positive values of eddy energy are still indicated. This would suggest, as one might expect from theory, the transfer of energy from turbulent motions of still larger scales or the diffusion of eddy energy from elsewhere in the field of flow, or both.

The trends of the two classifications can be made to coincide, approximately, by assigning a value of K_H/K_m different from unity to one or the other, as shown in Fig. 3, where the ratio was made equal to 5 for the over-land trajectories. However, the data are not sufficiently sensitive for that ratio to be accurately delineated. Rather, about all

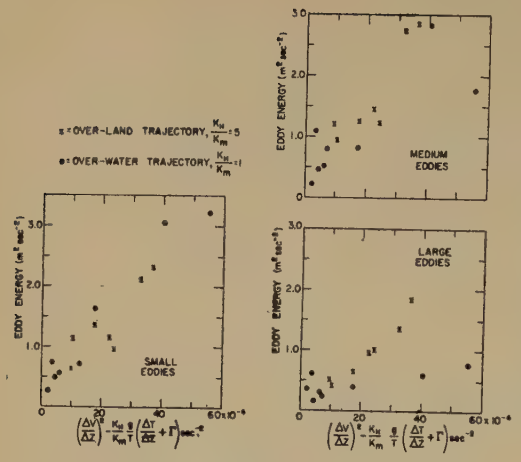

x = OVER-LAND TRAJECTORY, $K_H/K_m = 5$
• = OVER-WATER TRAJECTORY, $K_H/K_m = 1$
EDDY ENERGY ($m^2 sec^{-2}$)
SMALL EDDIES
MEDIUM EDDIES
LARGE EDDIES
$\left(\frac{\Delta V}{\Delta Z}\right)^2 - \frac{K_H}{K_m}\frac{g}{T}\left(\frac{\Delta T}{\Delta Z}+\Gamma\right) sec^{-2}$

Figure 4. Variation of eddy energy with energy supply by eddy size.

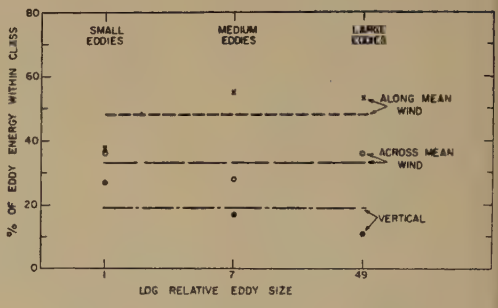


Figure 5. Partition of mean eddy energy.

that one can say is that the ratio appears to be about 3 to 6 times greater for air travelling over land than over water. This conclusion derives some measure of justification from the theory of discriminatory transfer formulated by Priestley and Swinbank (1947). Because of buoyancy effects, the theory implies that during unstable conditions one might expect K_H to be greater than K_m. If the critical Richardson number can be considered as a measure of K_m/K_H, Wanta (1953) has found it to be less than unity in the 46 m – 125 m layer at Brookhaven immediately following the transition from 'smooth' to turbulent flow. Since the time spent over land in the over-water cases was comparatively short, the greater probability of fully developed buoyancy effects at 91 m due to heating from below lies with the over-land trajectories.

5. Spectral analysis

By means of a simplified method of spectral analysis described by Panofsky (1952a) in which moving averages are utilized as frequency filters, three classes of eddies were defined whose approximate period-ranges are as follows :

Class	Period-range
small eddies	20 sec – 2·5 min
medium eddies	2·5 min – 17·0 min
large eddies	17·0 min – 57 min* / 114 min

This filtering permits a more detailed examination of the variation of eddy energy with the rate of energy supply, as illustrated in Fig. 4. The energy in each class of the 15 runs is plotted against the 1-hr average rate of energy supply as before; the K_H/K_m value of 5 was retained for the over-land cases. The picture for the small eddies is about the same as that shown in Fig. 3 in that the trends are nearly identical. However, with increasing eddy size, the trends become increasingly divergent so that in the class of largest eddies, the difference is quite marked. It is conceivable that this phenomena could be a manifestation of the smaller range in size of the roughness elements of a water surface compared to the range found over land.

* When the u_i were defined in terms of a line of regression.

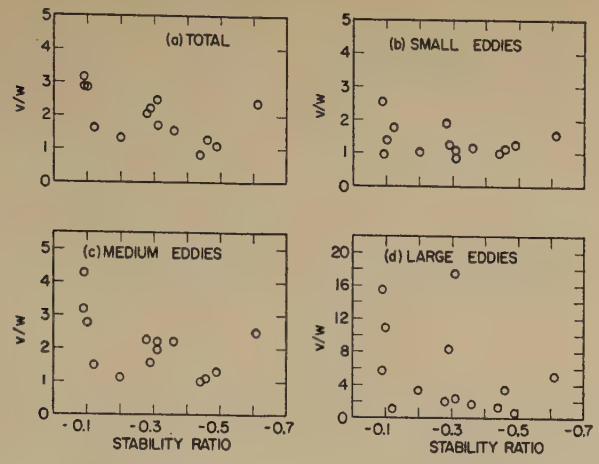


Figure 6. The variation of cross-wind to vertical eddy energy with ' stability ratio.'

The average partition of the total eddy energy is represented in Fig. 5 by the three horizontal lines, and the average partition of the eddy energy within each class by the points. The degree of vertical separation of the lines and points is a measure of the inequality of the partition. In the vertical there is a regular variation with eddy size. If measured by the range of the extremes, the tendency away from equipartition does increase with eddy size as hypothesized. On the other hand, if the trends shown are reliable, equipartition of energy among all components is suggested at smaller eddy sizes, and in the horizontal at larger sizes than those investigated. Significant differences were not found in the average partition among over-land or over-water cases, and no classification on that basis is shown in the figure. Within each class of eddies, the energy in the horizontal motions was reasonably independent of the total energy, but the proportion in the vertical motions decreased as the total energy increased.

The ratio of cross-wind to vertical eddy energy, which is of particular importance in atmospheric diffusion, is shown in Fig. 6 as function of the ' stability ratio ' for the unstable cases. This ratio was defined as the temperature at 125 m, minus the temperature at 11 m, divided by the square of the wind speed at 91 m. Although the scatter of the points is considerable, the expected tendency toward equipartition with more negative values of the stability ratio appears to be borne out. The comparatively large ratios of cross-wind to vertical energy in large eddies are a manifestation of the limit imposed by the ground on the development of large eddies in the vertical whereas the motions in the horizontal plane are not similarly restricted.

6. Other properties of eddy motions at 91 m

Since the statistics of eddy motion at comparable elevations are particularly meagre, three of the more important parameters which involve a minimum of corollary assumptions in their determination are given in Table 1. The generality of their orders of magnitude is naturally limited by the conditions of observation, e.g., stability range, scale of measurements, surface roughness, etc., but the values may serve as useful guides in future theoretical considerations of elevated layers.

(a) *The intensity of turbulence*

These data, also called ' gustiness ' factors, were given by

$$g_i = \frac{\sqrt{(\overline{u_i'^2})}}{\bar{u}}$$

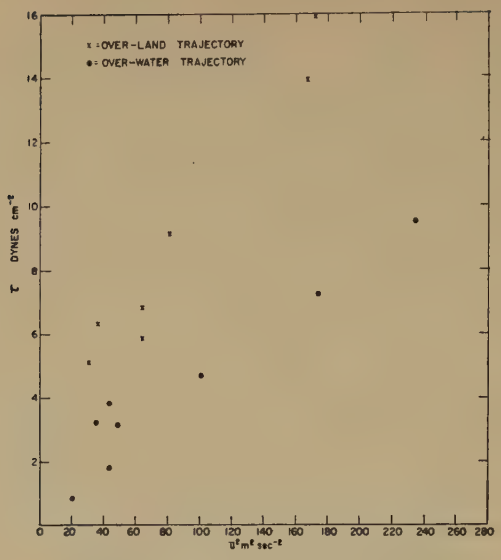


Figure 7. The variation of τ with u.

where the primed quantities are defined by Eqs. (1) or (2) and $\bar{u}$ is the mean velocity in the direction of the mean wind at 91 m. The range of values, from 0·06 to 0·21, is in line with those previously reported at lower elevations. While g_x and g_y appear to be independent of the wind speed, a tendency for g_z to decrease with increasing $\bar{u}$ is indicated. In general the gustiness components of the over-water cases were lowest, but there is no clear-cut classification on the basis of trajectory. Goldie (1925) found gustiness differences between what he called Polar and Equatorial air masses which he ascribed to stability differences in the two air-mass types. Presumably the stability range of the cases studied here was smaller than that considered by Goldie, but within this range there was little apparent correlation between the intensity of turbulence and the vertical temperature gradient in the lower 125 m.

(b) *The Reynolds stress*

This parameter,

$$\tau = -\rho \overline{u' w'}$$

was evaluated assuming constant density ($1{\cdot}25 \times 10^{-3}$ gm cm^{-3}), the primed quantities again being defined by the appropriate Eq. (1) or (2). From velocity-profile theory, a linear relation between the stress and the square of the wind speed would be expected and might be inferred from the array of points in Fig. 7. The classification of the data by trajectories is again clearly evident indicating a smaller degree of 'coupling' or momentum transfer at all winds speeds in the air which had come from over the ocean. As has been previously noted (Panofsky and McCormick 1952) the magnitudes of the stresses computed at this elevation are somewhat higher than those previously reported. It has been shown that the technique used for the determination of the Reynolds stress leads to the correct order of magnitude (Panofsky 1952b), hence the values must be considered in the light of the greater turbulence present during unstable conditions.

(c) *Eddy viscosity*

Neglecting molecular viscosity, the exchange-coefficient hypothesis implies that the Reynolds stress can be expressed as the product of the momentum-exchange coefficient

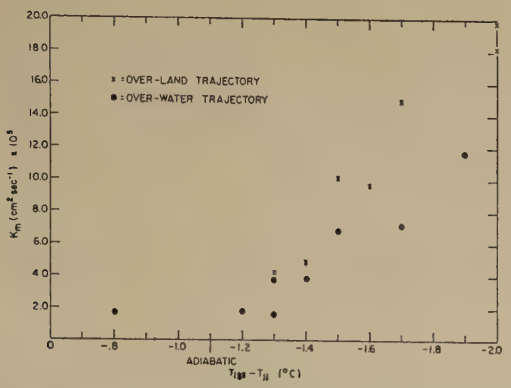


Figure 8. Variation of K_m with the temperature difference between 125 m and 11 m.

and the gradient of the mean wind, or

$$\tau = \rho K_m \frac{d\bar{u}}{dz}$$

The values of K_m shown in Table 1 utilized the power law proposed by Deacon (1949) which Panofsky and Singer (1951) have shown may reasonably describe the velocity profile at Brookhaven. This law can be expressed,

$$\frac{d\bar{u}}{dz} = \frac{u_*}{k\, z_0^{1-\beta}}\, z^{-\beta}$$

where

$u_* = \sqrt{\tau/\rho}$

k = von Karman's constant ($\simeq 0{\cdot}4$)

z_0 = a ' roughness length '

β = a function of the temperature gradient

Panofsky and Singer's regression curve of β against the 125 m-11 m temperature difference, and average value of z_0 (100 cm) were used in computing the K_m. Of particular interest is the good correlation of the eddy viscosity with the temperature difference between 125 m and 11 m in the unstable cases as shown in Fig. 8. While a correlation was not unexpected for the scale of motions involved, the apparent linear relationship is worthy of note. The eddy viscosities were sensibly independent of both the wind speed and the shear, hence the smaller stresses noted in the over-water runs would appear to be associated primarily with their comparatively greater stability.

7. Conclusions

Briefly summarized, the primary conclusions which can be derived from these observations which, with one exception, are representative of the most probable daytime conditions at Brookhaven, include the following :

(a) The magnitude of the turbulent energy is proportional to the rate of energy supply as given by Richardson's expression within the scales of measurement adopted. However, the proportionality varies with the scale of motion and the trajectory of the air reaching Brookhaven.

(b) There are at least two 'ground effects'; one is that within the period range of 20 sec to about one hour, the partition of eddy energy becomes increasingly unequal with increasing eddy size, mainly because of a decrease in the fraction of the total energy represented in the vertical motions. The other effect is that at a given frequency, as the total eddying energy increases, the fraction represented by the vertical motions decreases.

(c) Within the stability-range sampled, the relative eddy energies of the horizontal components were not correlated with the vertical temperature and velocity gradients measured on the tower. Presumably, this partition is a function of scale effects other than those which can be measured at a single point.

(d) Two turbulence regimes apparently exist, at 91 m at Brookhaven during unstable conditions, whose characteristics depend on whether the past trajectory of the air has been primarily over-land or over the ocean. The casual factors are not yet clear.

Acknowledgments

This research was performed under the auspices of the Atomic Energy Commission. In preliminary form this paper was read at the 120th National Meeting of the American Meteorological Society, 26-29 January 1953, New York City.

The author is grateful to the Meteorology Group of the Brookhaven National Laboratory for their generous co-operation in obtaining the observations, and to Mr. R. C. Wanta and Dr. H. A. Panofsky for their advice and suggestions throughout the project. Thanks are also due to Mr. M. Lefkowitz and to Mr. G. A. DeMarrais for their assistance in reducing the basic data and performing the computations.

References

Best, A. C.	1935	*Geophys. Mem. Met. Off.*, No. 65.
Deacon, E. L.	1949	*Quart. J. R. Met. Soc.*, **75**, p. 89.
Goldie, A. H. R.	1925	*Ibid.*, **51**, p. 357.
Mazzarella, D. A.	1952a	*Bull. Amer. Met. Soc.*, **33**, p. 60.
	1952b	Personal communication.
Ower, E.	1949	*The Measurement of Air Flow*, London (Chapman and Hall).
Panofsky, H. A.	1952a	*Sci. Rep. Penna. State Coll. Div. Met.*, No. 1.
	1952b	*Quart. J. R. Met. Soc.*, **78**, p. 411.
Panofsky, H. A. and McCormick, R. A.	1952	*Geophys. Res. Pap.*, Cambridge, Mass., No. 19, p. 219.
Panofsky, H. A. and Singer, I. A.	1951	*Tech. Inf. Serv.*, Oak Ridge, Tenn., NYO-1559.
Priestley, C. H. B. and Swinbank, W. C.	1947	*Proc. Roy. Soc.* A, **189**, p. 543.
Scrase, F. J.	1930	*Geophys. Mem. Met. Off.*, No. 52.
Singer, I. A. and Smith, M. E.	1953	*J. Met.*, **10**, p. 121.
Sutton, O. G.	1953	*Micrometeorology*, New York (McGraw-Hill).
Taylor, G. I.	1927	*Quart. J. R. Met. Soc.*, **53**, p. 201.
Taylor, R. J.	1952	*Geophys. Res. Pap.*, Cambridge, Mass., No. 19, p. 231.
Wanta, R. C.	1953	*Quart. J. R. Met. Soc.*, **79**, p. 398.

551.551 : 551.557

Turbulence in the upper air, as shown by radar-wind and radiosonde measurements

By F. J. SCRASE
Meteorological Office, Harrow, Middlesex

(Manuscript received 21 April 1953, in revised form 7 May 1954)

SUMMARY

Observations of the fluctuations, minute by minute, of wind components and temperature in a radiosonde and radar-wind ascent up to 30 km were analysed. After allowing for the effects of random errors a standard deviation of about 4 kt for the eddy velocity, with a mean wind of about 25 kt, was obtained. The standard deviation of the temperature fluctuations was 0·85°F in the troposphere but decreased to about one-third of this in the upper stratosphere.

1. INTRODUCTION

In an upper-wind sounding the plotted positions of a balloon observed at equal intervals of time fluctuate about the smooth track which the balloon would follow if it were moving with the mean velocity of the wind. There are similar variations in the rate of ascent. The fluctuations are due partly to turbulence and partly to errors of observation. If we know the magnitude of the latter we can obtain some information about the turbulent fluctuations. Harrison (1948a) has given detailed information about the errors of observation of radar-wind equipment which enable this to be done.

Temperature observations by radiosonde can be treated similarly since Harrison (1948b) has also obtained estimates of observational error in the temperature readings. The turbulent fluctuations of temperature, however, are smoothed to some extent by the lag of the temperature element.

It seemed worth while applying Harrison's estimates of the errors of observation to one of a series of high-altitude ascents to see whether it would yield any useful information on the scale of turbulence at levels up to 100,000 ft.

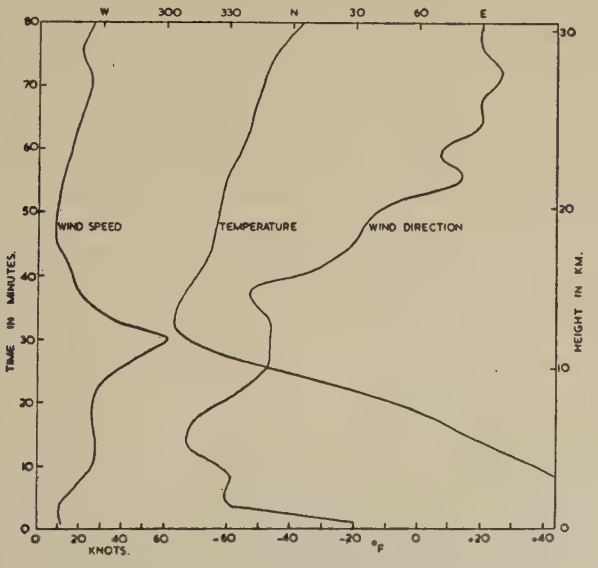


Figure 1. Upper-air sounding at Downham Market at 22 GMT on 29 June 1949.

2. Reduction of the observations

The sounding chosen for this investigation was one made from Downham Market (52°36′N, 0°24′E) on 29 June 1949, at 22 GMT and described in a previous paper (Scrase 1949). The synoptic conditions at the time were anticyclonic, there being a high-pressure area over Scotland. The smoothed measurements of wind speed, direction and temperature plotted against height are reproduced in Fig. 1, from which it will be seen that there was considerable wind shear above and below the tropopause. Radar observations of slant range R, azimuth A and elevation E of the balloon were taken at minute intervals simultaneously with temperature readings from the radiosonde.

(a) *Wind measurements*

In order to compare the components of eddy velocity the distances x_n, y_n and z_n the balloon travelled between one minute and the next in the W to E, S to N and vertical directions were first computed from the radar readings thus :

$$x_n = R_n \cos E_n \sin A_n - R_{n-1} \cos E_{n-1} \sin A_{n-1}$$
$$y_n = R_n \cos E_n \cos A_n - R_{n-1} \cos E_{n-1} \cos A_{n-1}$$
$$z_n = R_n \sin E_n - R_{n-1} \sin E_{n-1}$$

where the suffixes $n - 1$ and n indicate observations at consecutive minutes.

These displacements are plotted against time (and height) in Fig. 2 and their deviations from the smooth curves drawn through them represent the apparent (i.e., uncorrected for errors of observation) W-E, S-N and vertical components of eddy velocity, u_n, v_n, w_n, for one-minute intervals. Thus $u_n = (x_n - \bar{x})/t$; $v_n = (y_n - \bar{y})/t$; $w_n = (z_n - \bar{z})/t$ where $\bar{x}$, $\bar{y}$ and $\bar{z}$ are the smoothed (mean) values and t is the time interval. The smooth curves in Fig. 2 were drawn so as to make the algebraic means of the deviations, over long periods, negligible. The deviations themselves are plotted in Fig. 3, and since the long-period drifts that are evident in Fig. 2 are eliminated, the minute-to-minute fluctuations are more readily comparable.

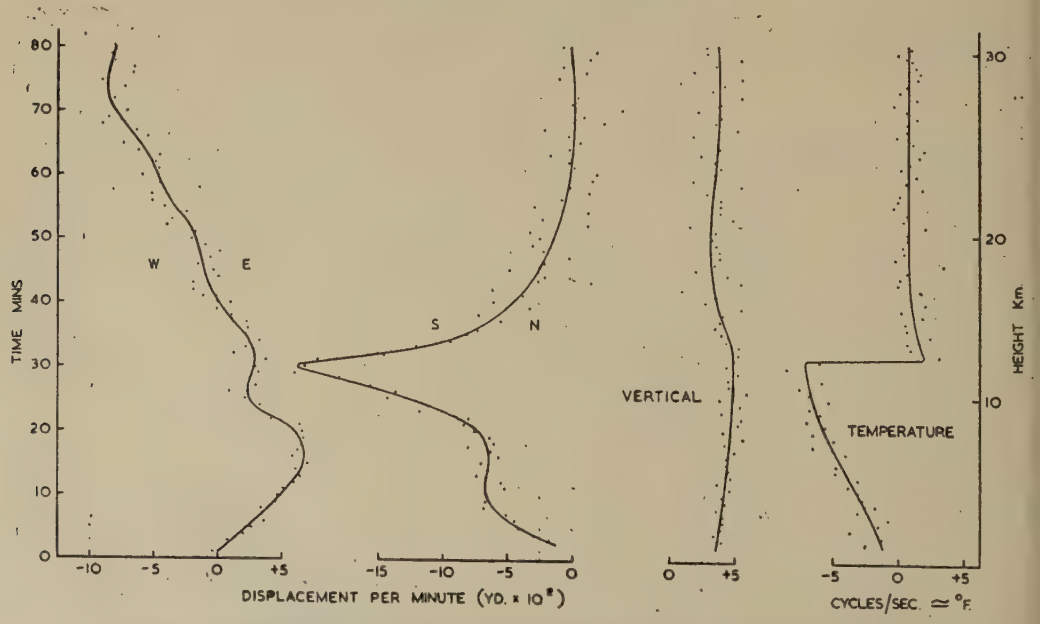


Figure 2. Observed displacements of balloon and observed temperature differences in one-minute intervals.

TABLE 1. STANDARD DEVIATIONS OF WIND COMPONENT AND TEMPERATURE FLUCTUATIONS FROM MINUTE-TO-MINUTE AT DOWNHAM MARKET AT 20 GMT ON 29 JUNE 1949.

Time from start (min)	3-28	29-54	55-80
Height (km)	1-12	12-21	21-31
Mean wind speed (kt)	29	23	20
Mean rate of ascent (kt)	13	12	11
Mean wind direction	340°	020°	090°
Horizontal component	kt (1 kt = 0·515 m/sec)		
Observed $(u_n^2 + v_n^2)^{\frac{1}{2}}$	2·8	5·3	6·1
Random error $(\delta u^2 + \delta v^2)^{\frac{1}{2}}$	1·2	2·9	3·4
Corrected $(u^2 + v^2)^{\frac{1}{2}}$	2·6	4·5	5·0
Vertical component			
Observed w_n	1·5	3·0	3·6
Random error δw	1·0	2·6	2·8
Corrected w	1·1	1·5	2·2
Vector resultant			
$(\overline{u^2} + \overline{v^2} + \overline{w^2})^{\frac{1}{2}}$	2·8	4·7	5·5
Temperature fluctuations		°F	
Observed (T_n)	0·94	0·67	0·45
Random error (combines height-interval error and temperature error) (δT)	0·48	0·41	0·41
Corrected for random error	0·81	0·53	0·17
Corrected for random error and lag (T)	0·85	0·64	0·29

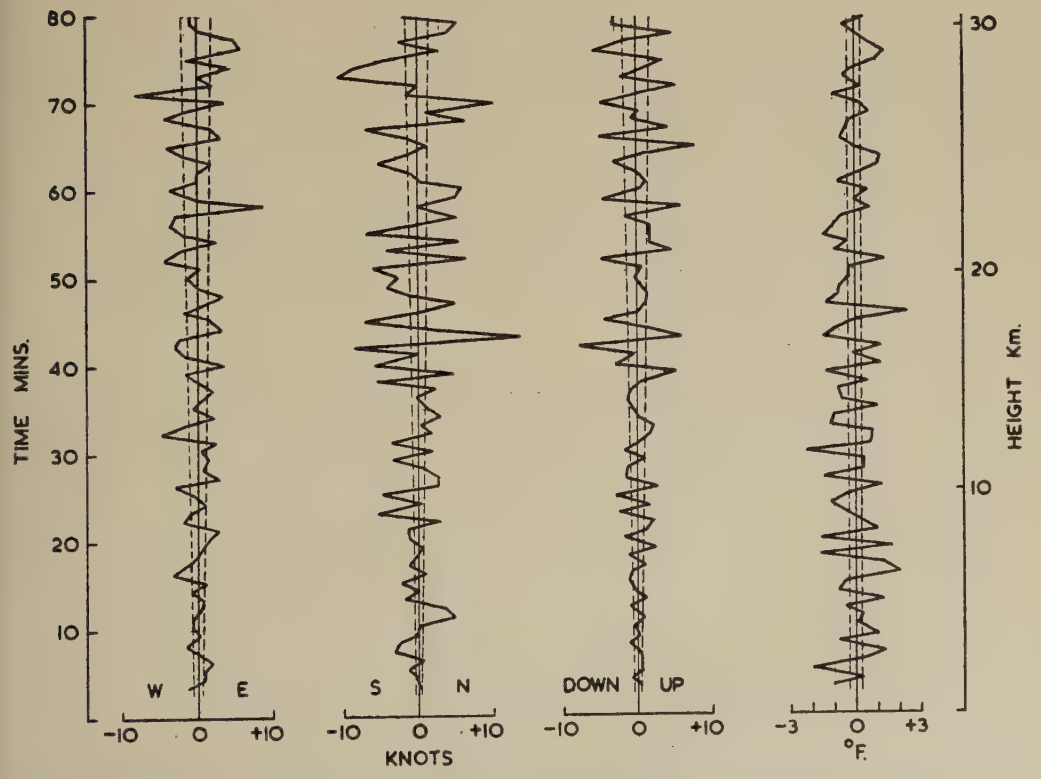


Figure 3. Observed deviations of velocity components and temperature differences from mean curves (dotted lines indicate the probable error of individual observations).

The standard deviations of the observed fluctuations were computed for each of the three parts of the sounding covered in the periods 3 to 28, 29 to 55 and 56 to 80 min from the start and representing the troposphere, the lower stratosphere and the upper stratosphere. The values, each of which was derived from 27 observations, are given in Table 1. We now require to know to what extent these values are due to errors of observation.

Harrison's (1948a) investigation of the random errors of radar readings with G.L.III equipment showed the r.m.s. values of the errors to be :

Error in slant range, δR, = 16 yd

Errors in azimuth and elevation, δA and δE, = 0·10° = 0·0017 radian.

Using these figures the random errors in the components of eddy velocity derived from the readings can be computed from the expressions :

$$\overline{(\delta u)^2} + \overline{(\delta v)^2} = 2\left[\overline{(\delta R)^2}\cos^2 E + \overline{R^2}\sin^2 E\,\overline{(\delta E)^2} + \overline{R^2}\cos^2 E\,\overline{(\delta A)^2}\right]/t^2$$

$$\overline{(\delta w)^2} = 2\left[\overline{(\delta R)^2}\sin^2 E + \overline{R^2}\cos^2 E\,\overline{(\delta E)^2}\right]/t^2$$

These errors were computed for the positions of the balloon at every fifth minute and r.m.s. values for the three parts of the sounding obtained. They are given in Table 1 together with the standard deviations adjusted for these errors by means of the relationships of the type :

$$\overline{(u)^2} = \overline{(u_n)^2} - \overline{(\delta u)^2}$$

The table also includes values of the resultant vector deviation given by :

$$\overline{r^2} = \overline{u^2} + \overline{v^2} + \overline{w^2}$$

It is relevant to inquire (a) to what extent Harrison's values of the errors can be taken as representative of the sounding now under investigation and (b) what difference would result if the errors in this sounding were appreciably different from Harrison's values. His radar errors were derived from 16 comparisons of two radar sets and while they could be regarded as representing the standard of accuracy obtainable under good, if not the best, conditions, they would not necessarily apply to an individual sounding. The errors for the individual soundings in Harrison's series varied within a range of about 0·65 to 1·50 of the mean values for the series. For the sounding now under investigation there was, therefore, at least a 1 in 16 chance of the errors being 1·5 times as great as the values we have used.

Harrison has pointed out, however, that the random errors could not have exceeded the observed standard deviations given in Table 1. For example, in the 21-31 km height interval, the random error for the vertical component could not have been greater than 3·6/2·8, i.e. 1·3 × the value based on Harrison's data. Since, at the long ranges, this error depends mainly on the elevation error it is evident that this could not have been greater than 0·13°. For the same height interval the random error of the horizontal component, which depends on the azimuth as well as the elevation error, could not have exceeded 6·1/3·4, i.e., 1·8 × the value used in Table 1. Assuming that the errors during the sounding were not more than 1·5 × as great as the values used in Table 1 we can readily obtain lower limits for the standard deviations of the turbulent fluctuations. For the vertical component at all three height intervals and for the horizontal component in the two higher intervals these would become insignificant In the other case, viz., that of the horizontal component in the lowest height interval the corrected standard deviation would not have been less than 2·4 kt, which is only slightly smaller than the more probable value in Table 1.

A further question affecting the observed fluctuations is how much does the inertia of the balloon reduce its response to a change in air speed. It can be calculated that a balloon of the size used (2 kg), and with the load carried (also 2 kg), should acquire 90 per cent of the velocity of a sudden gust in a few seconds and it can be expected to follow fluctuations having a period of the order of a minute without appreciable reduction of amplitude.

(b) *Temperature measurements*

The temperature observations, at the minute intervals, were obtained initially as radiosonde frequencies in cycles/sec. A change of frequency of 1 c/sec corresponds roughly to a change of temperature of 1°F. The differences between successive readings are plotted in Fig. 2. In addition to the fluctuations due to turbulence and to random errors of temperature measurements they include the effect of variations in the rate of ascent; with a vertical gradient of temperature these produce corresponding variations in the temperature differences observed at fixed time intervals. The deviations from the mean curve which are plotted in Fig. 3 are, however, corrected for these variations (the magnitude of their effect depends on the temperature gradient). The standard deviations for the three parts of the sounding, when divided by $\sqrt{2}$ and multiplied by the radiosonde calibration factor, represent the observed temperature fluctuations. They are given in Table 1. Harrison's (1948b) investigation of radiosonde errors give the r.m.s. random error of a temperature reading as 0·40°F. We have also to take account of the random error in the measurement of the rate of ascent which, with the temperature gradients observed, cause a random error in the temperature deviations derived from the successive differences of 0·25°F in the troposphere and 0·11°F in the stratosphere. The standard deviations corrected for the combined random error are also given in Table 1.

The temperature observations are also subject to errors due to the lag of the bimetal element. For the rate of ascent used the lag coefficient at ground level is 7 sec and as it probably varies inversely as the square root of the air density the mean coefficients for the three parts of the sounding are approximately 10, 20 and 40 sec. The effect of lag on the measurement of the temperature fluctuations can be readily allowed for if they are treated as being sinusoidal. There is, in fact, some indication of a rough periodicity of between 3 and 4 min in both the temperature and the wind observations. The effect of a lag coefficient λ is to reduce the true amplitude A of a sinusoidal variation of period P to $A/(1 + 4\pi^2 \lambda^2/P^2)^{\frac{1}{2}}$. The standard deviations have been corrected on this basis, using the values of λ given above and taking P to be 200 sec; the corrected values are included in Table 1.

Table 1 shows that the observed standard deviation in the 21-31 km interval was 45/41, i.e., 1·1 × the random error derived from Harrison's mean value. It is safe to assume this factor as maximum for the other height intervals as well; we can therefore apply it to obtain minimum values for the temperature fluctuations. For the lowest height interval the corrected standard deviation is not less than 0·81°F and for the 12-21 km interval it is not less than 0·59°F. These minimum values are not appreciably different from the more probable values given in Table 1.

3. Aerodynamic effects

It will be seen from Fig. 2 that the smoothed curve for the rate of ascent of the balloon shows the usual decrease with height after the tropopause is reached. This is generally attributed to the effect of the change in lapse rate on the difference in temperature between the gas in the balloon and the air outside. In the present ascent and similar high-altitude soundings with 2 kg balloons the decrease in rate of ascent ceases sharply

at about 18 km (60,000 ft). This is almost certainly due to a change in the drag coefficient which is associated with the decrease in Reynolds number as the balloon ascends. The drag coefficient is $C_d = 8\,Lg/\rho W^2\,\pi d^2$ and Reynolds number is $Re = W\rho d/\mu$, where L is the free lift, W the rate of ascent and d the diameter of the balloon, and ρ is the density and μ the viscosity of the air. In Fig. 4 computed values of the drag coefficient are plotted against the logarithm of Reynolds number for comparison with corresponding data quoted by Goldstein (1938) for solid spheres in wind tunnels and dropping in free air. It might be expected that in the steeply changing part of the drag coefficient curve there would be variations in the drag, e.g., due to changes in the shape of the balloon, which would result in increased fluctuations of the balloon speed. The observed fluctuations are, however, not noticeably larger in this part of the ascent than in the parts where the drag coefficient is not changing so much. It seems reasonable to assume, therefore, that the fluctuations that remain after the observational errors are eliminated are the effect of turbulence in the atmosphere and not turbulence set up by the air flow round the balloon.

4. Eddy velocities and temperature fluctuations

It will be seen from Table 1 that in all cases the observed standard deviations of wind components exceeded the random errors computed from Harrison's data but the differences are not large for the vertical component. The fluctuations in the vertical component were smaller than the horizontal fluctuations at all heights. In the troposphere the corrected standard deviation of the horizontal fluctuations was about 10 per cent of the mean wind speed. This is about two-thirds of the ratio found by Giblett *et al.* (1932) for the horizontal component of surface wind at Cardington using 5-sec means. Other data for comparison include a series of observations on smoke puffs at heights varying between 2,000 and 27,000 ft from which Durst (1948) obtained vector standard deviations (in the horizontal plane) ranging from 0·5 to 3·5 kt, the average being 1·5 kt for an average wind speed of 18 kt; the downwind and crosswind components were practically the same size. Durst and Gilbert (1950), from observations of constant-height balloons drifting at about 20,000 ft in winds averaging about 50 kt, obtained fluctuations of horizontal wind of the order of 3 or 4 kt from one half-hour to another. Some results of Rapp (1952), who analyzed upper-air soundings made with U.S. Rawin set AN/GMD-I, indicated that up to 8 km, with winds increasing from about 10 to 50 kt, the r.m.s. errors in the observations of horizontal wind varied between 1 and 2 kt and the

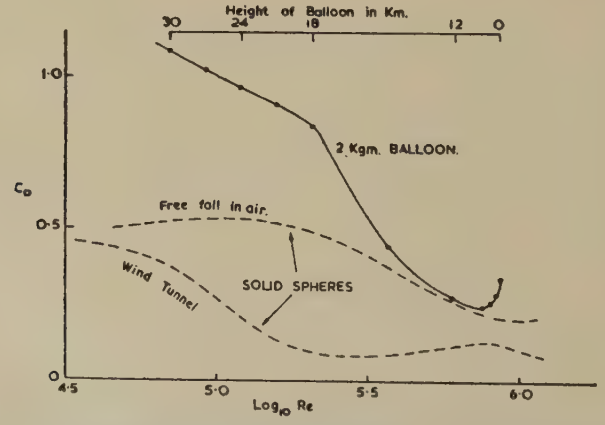


Figure 4. Variation of drag coefficient and Reynolds number. The curves for solid spheres are based on data given by Goldstein (1938).

standard deviation of the fluctuations of the wind varied from about 1 to 5 kt. Voss (1950) observed fluctuations in rates of ascent in radiosonde observations which were greater than observational errors and confirmed their magnitude by using radiosonde heights computed from simultaneous pressure and temperature measurements. He obtained, in clear air up to 20 km (65,000 ft), maximum vertical velocities of about 4 kt, the periods of the fluctuations averaging about 3 min.

The periods of the wind fluctuations observed in the Downham Market sounding were practically the same for each component and were roughly 3 to 4 min. Oscillations of about this period, in which the up and down currents were about 5-10 kt, were recently reported by an aircraft flying at 8,500 ft near the West of Scotland (Turner 1952). A similar period of oscillation was clearly shown by some radar observations by Warner and Bowen (1953) of the fall of a bundle of metal foil in clear air at a height of about 3 km. The path of the bundle was observed over a period of 8 min at intervals of about 20 sec and it showed a smooth oscillation with a period of about 3·5 min and an amplitude about the rate of fall in still air of about $\pm$ 5 kt. In the Downham Market sounding the period of oscillation would correspond to eddies having horizontal dimensions of about 10,000 ft and vertical dimensions of 5,000 ft. In Durst's (1948) smoke-puff observations the periods of the fluctuations were 5 to 15 min and the eddies extended horizontally 2 to 10 mi and vertically 5,000 ft or more. Voss's observations indicated eddy dimensions of about 6,000 ft horizontally and 2,500 ft vertically. The Cardington measurements of surface wind showed the major eddies to be elongated in the downwind direction and to be about 4,000 ft long and 600 ft wide but there were other groups of major eddies, in certain conditions, which had dimensions in the direction of the wind up to 10, and even 20, mi.

The maximum minute-to-minute velocities measured in the Downham Market sounding were about 15 kt for the horizontal component and about 8 kt for the vertical component; they occurred in the layer between 30,000 and 60,000 ft. The maximum minute-to-minute accelerations were 0·02 g for the horizontal and 0·01 g for the vertical components.

Turning now to the temperature fluctuations it will be seen from Table 1 that in the upper stratosphere the standard deviation is not much larger than the random error but in the troposphere it is roughly twice as large as the error and after allowing for this and for lag a value of 0·85°F is obtained. The corresponding value for the lower stratosphere is 0·64°F. Fluctuations of this order of magnitude have been reported by Frith (1950) and by Grant (1951) from aircraft observations in the troposphere and in the lower stratosphere. The period of the temperature fluctuations observed at Downham Market was about the same as for the wind fluctuations.

5. Future work

The present sounding technique, with eye readings at minute intervals, is not designed for the measurement of turbulent fluctuations in the free atmosphere but this investigation indicates that with some improvements to reduce the observational errors useful measurements should be obtainable. The chief need is for a higher degree of angular resolution in the wind-finding equipment. Automatic tracking of the balloon should facilitate this and when combined with automatic recording of the data it should enable readings to be obtained at shorter time intervals. A considerable increase in the rate of response of the temperature element is desirable and analysis of the data would be facilitated by automatic computing of the wind components. Such improvements are, in fact, being incorporated in the radarsonde theodolite system now under development (Jones, Hooper and Alder 1951).

Acknowledgment

I am glad to acknowledge the assistance of Miss D. Chapman and Mrs. J. Seaton in the computational work of this investigation. The paper is published by permission of the Director of the Meteorological Office.

References

Durst, C. S.	1948	*Quart. J. R. Met. Soc.*, **74**, p. 349.
Durst, C. S. and Gilbert, G. H.	1950	*Ibid.*, **76**, p. 75.
Frith, R.	1950	*Air Ministry, Met. Res. Ctee., M.R.P.*, No. 552.
Giblett, M. A. and other members of the staff of the Meteorological Office	1932	*Geoph. Mem. Met. Off.*, London, No. 54.
Goldstein, S.	1938	*Modern Development in Fluid Dynamics*, **2**, Oxford (Clarendon Press), p. 495.
Grant, D. R.	1951	*Air Ministry, Met. Res. Ctee., M.R.P.*, No. 658.
Harrison, D. N.	1948a	*Ibid.*, No. 387.
	1948b	*Ibid.*, No. 422.
Jones, F. G., Hooper, J. E. N. and Alder, N. L.	1951	*Proc. I.E.E.*, **98** (II), p. 461.
Rapp, R. R.	1952	*New York Univ. Coll. Eng. Met. Pap.*, **2**, No. 1.
Scrase, F. J.	1949	*Met. Mag.*, London, **78**, p. 284.
Turner, H. S.	1952	*Ibid.*, **81**, p. 239.
Voss, H.	1950	*Ann. Met.*, Hamburg, **3**, p. 175.
Warner, J. and Bowen, E. G.	1953	*Tellus*, **5**, p. 36.

551.515.4 : 551.551 : 551.558.1

Five flights through a thunderstorm belt

By R. F. JONES
Meteorological Office, London

(Manuscript received 15 January 1954)

SUMMARY

Analysis of accelerometer and other aircraft-instrument records taken on five flights through thunderstorms shows that it is possible to obtain the detailed structure of the vertical air currents within the clouds. A feature of the clouds studied was the close proximity of upward and downward air currents of comparable magnitude, exceeding 20 ft/sec on occasions. Frequency and intensity of gusts increased markedly on entry into cloud and correlation with the radar echo received from the storms showed that all the biggest gusts were recorded within the echoing volume. On four out of five occasions when lightning was seen the aircraft was not in any appreciable upward or downward current, and on all five occasions the cloud had already attained its maximum development.

1. INTRODUCTION

A Meteor VII aircraft from the Aerodynamics Flight, Royal Aircraft Establishment has been used, in conjunction with the Meteorological Office radar station at East Hill, in the investigation of the turbulence within cumulonimbus clouds. This has been a continuation of a programme initiated with Spitfire aircraft and fully described elsewhere (Jones 1954). The change of aircraft type made it possible to carry an observer and to fit extra instruments. In particular the installation of an automatic recorder gave photographic readings at 10-sec intervals, of ICAN height, air temperature and indicated air speed. Vertical accelerations experienced by the aircraft were recorded continuously against a time base so that it should be possible by integration to obtain the speeds of movement of the aircraft in the vertical. Earlier attempts at integration, using accelerometer records obtained in Spitfire aircraft (Jones 1950), gave somewhat doubtful results because the cumulative effect of errors of measurement or calibration resulted in an impossibly large vertical speed of the aircraft at the end of the integration. With the recording of altimeter changes at 10-sec intervals it becomes possible to adjust the integrated accelerations to take account, by a further integration, of the actual displacement of the aircraft, every 10 sec if need be.

Five flights made on 13 June 1952 through a belt of thunderstorms have been analysed in this way and form the subject of this paper.

2. SYNOPTIC SITUATION AND FLIGHT PROGRAMME

The whole of England and Wales was covered by a shallow depression forming part of a low-pressure belt extending from Scandinavia to the Azores. The upper winds were variable and generally light up to 12,000 ft while the 500-mb and 300-mb contour charts showed the existence of a cold pool over SW England and the western English Channel. Conditions were therefore favourable for outbreaks of thundery rain or thunderstorms and these were confirmed by the extensive radar weather echoes visible at East Hill. Later in the day the echoes became less extensive and it was decided to investigate a belt, giving rise to a radar echo, to the north-east of East Hill. The radar echo tops reached to 35,000 to 36,000 ft, which was probably the tropopause height (1500 GMT tropopause heights – Larkhill 32,300 ft, Hemsby 39,000 ft).

The aircraft was in the vicinity of the clouds giving this belt of echo at 1525 GMT and described them as a line of five cumulonimbus clouds lying NW to SE. There was extensive anvil cirrus.

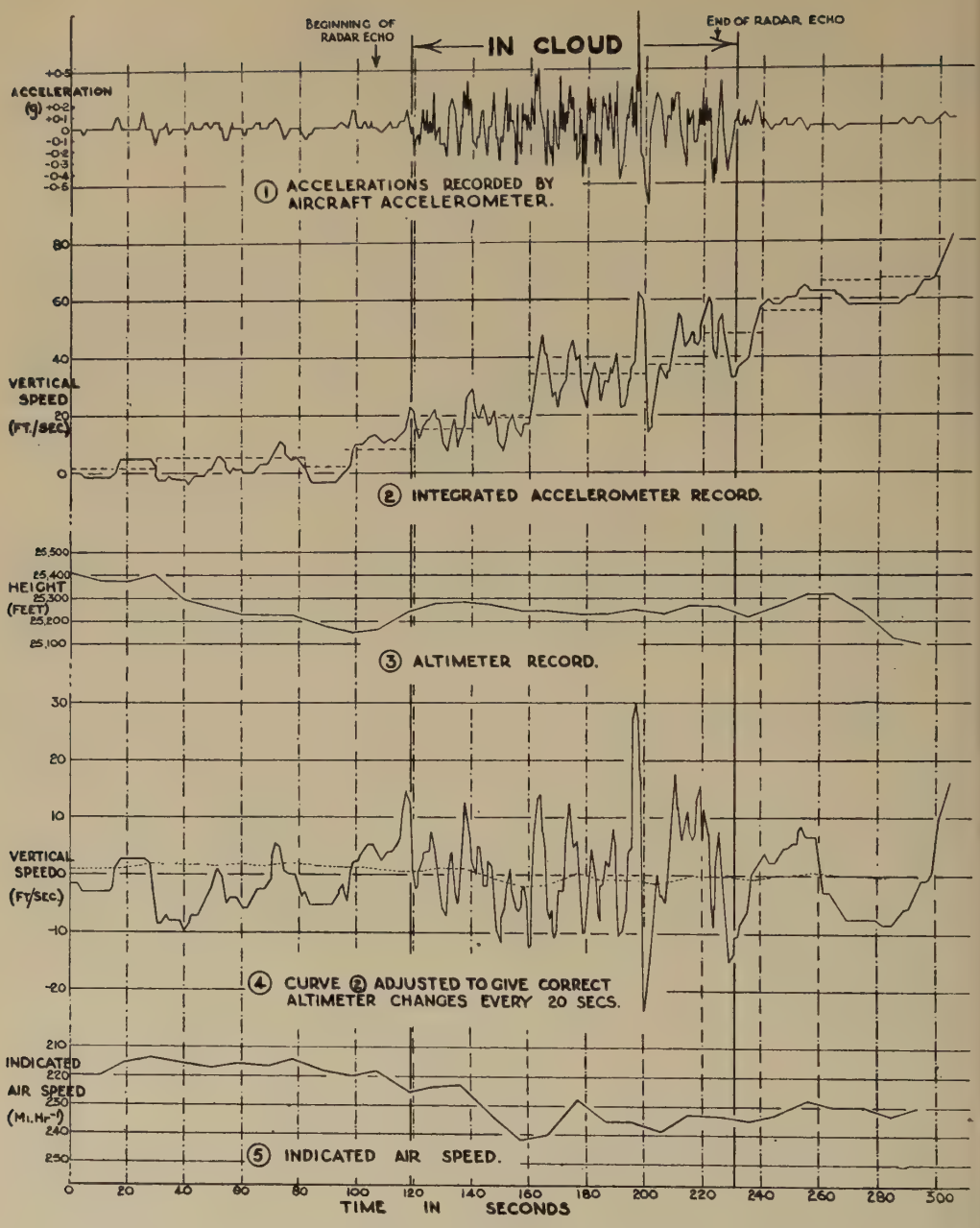


Figure 1. Cumulonimbus investigation – 13 June 1952 – Flight I at 25,200 ft.

The first two flights were made at 25,200 ft through the last but one of the clouds towards the south-east on headings of 270° Mag and 090° Mag and the three subsequent flights were made through the last cumulonimbus of all, on headings of 260°, 080° and 260° Mag at 20,200, 20,200 and 18,200 ft respectively. All the flights took place between 1528 and 1604 GMT. From the appearance of the radar echo it is certain that the clouds to SE of the belt were the most recently developed.

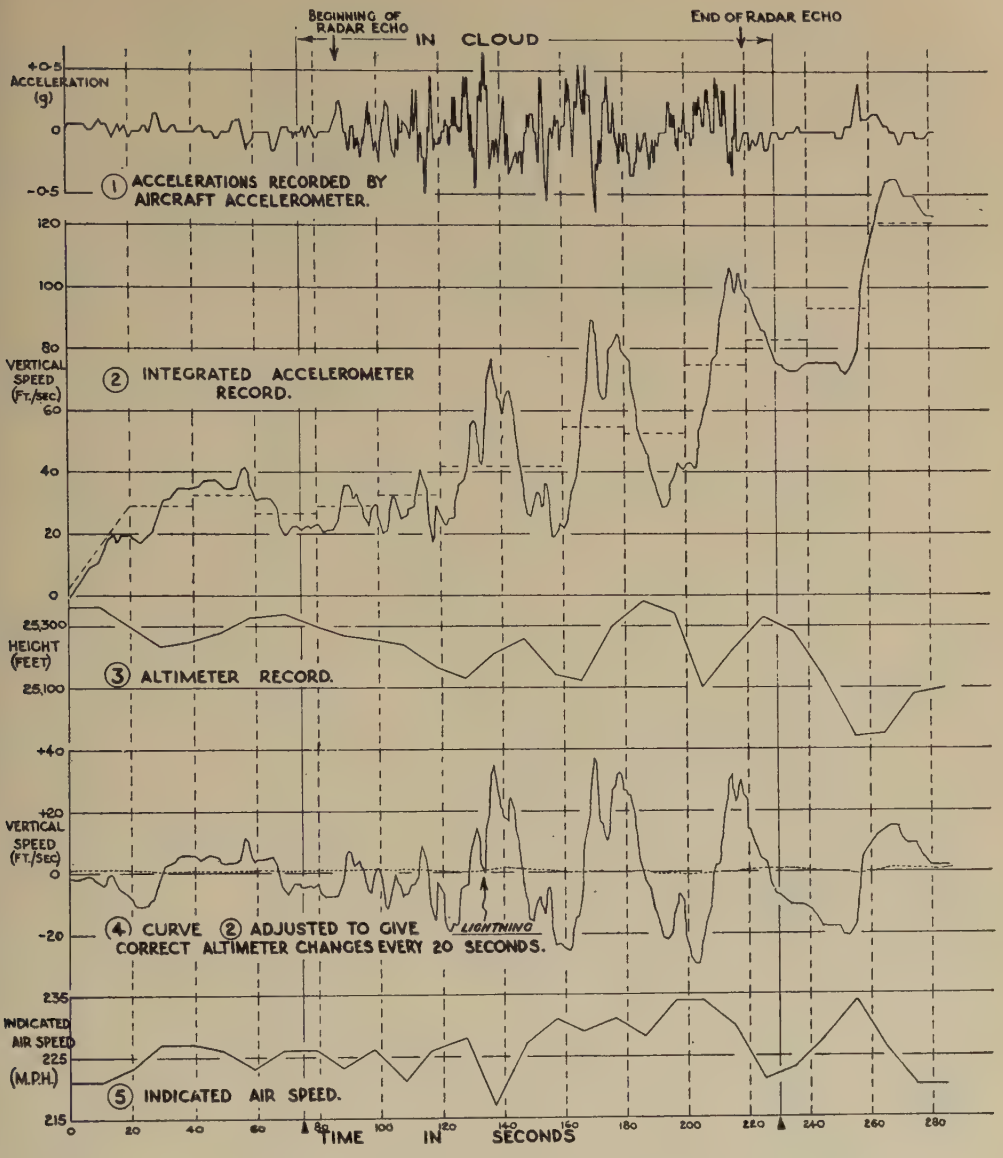


Figure 2. Cumulonimbus investigation – 13 June 1952 – Flight II at 25,200 ft.

3. Information available

For each of the five flights analysed the following information is available.

(i) A continuous record, on 35-mm film, of *accelerations* in the vertical experienced by the aircraft. The speed of the film was 1·29 cm/sec and the scale such that a vertical movement of the trace through 2·7 mm corresponded to an acceleration of 1 g (32 ft/sec^2). The film was examined under an enlarger and the accelerations recorded, to the nearest ·05 g, as often as necessary to give a faithful reproduction down to 0·1 sec. This detail with respect to time cannot be shown accurately in Figs. 1 to 5 but the time values have been used in the integrations.

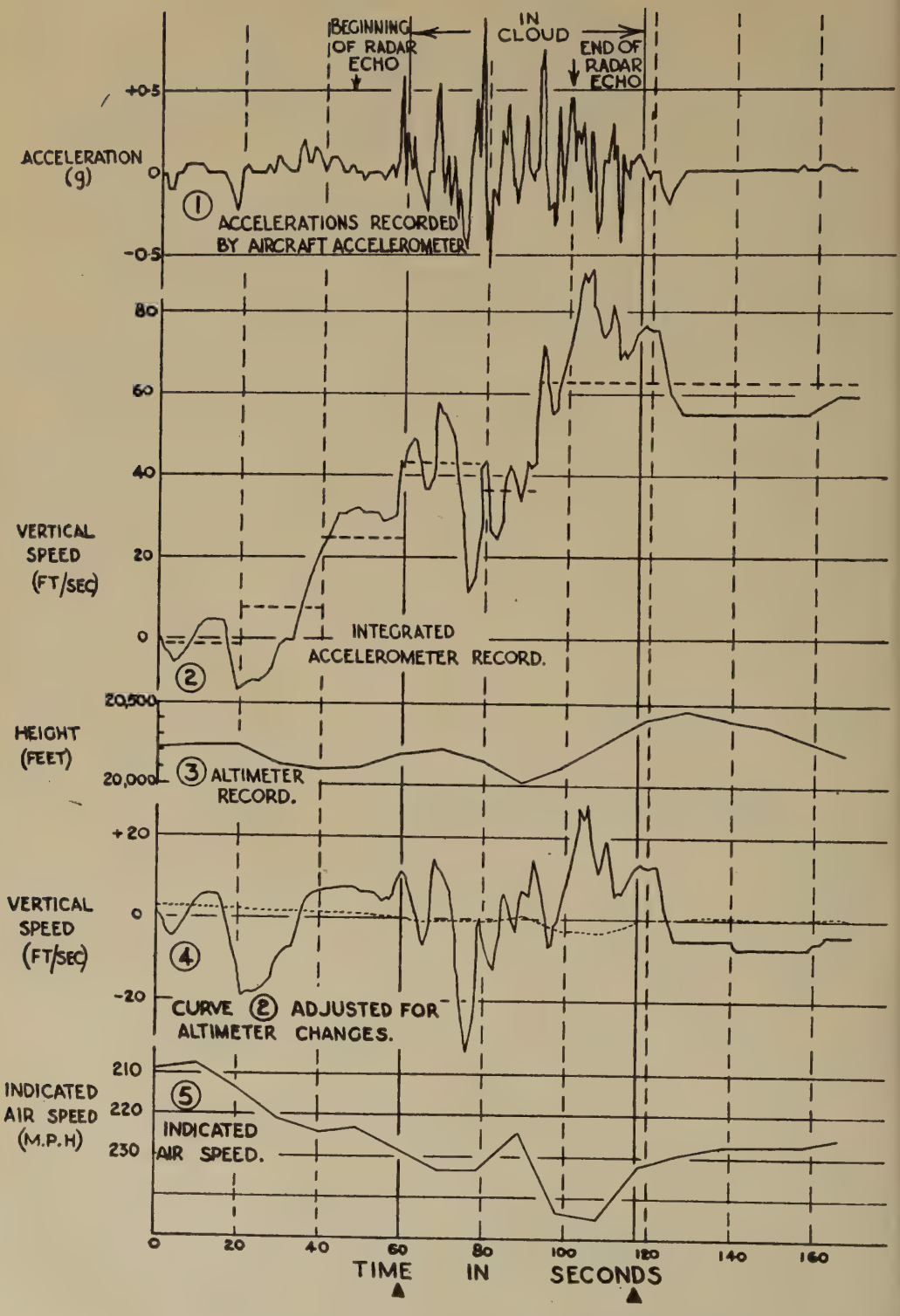


Figure 3. Cumulonimbus investigation – 13 June 1952 – Flight III at 20,200 ft.

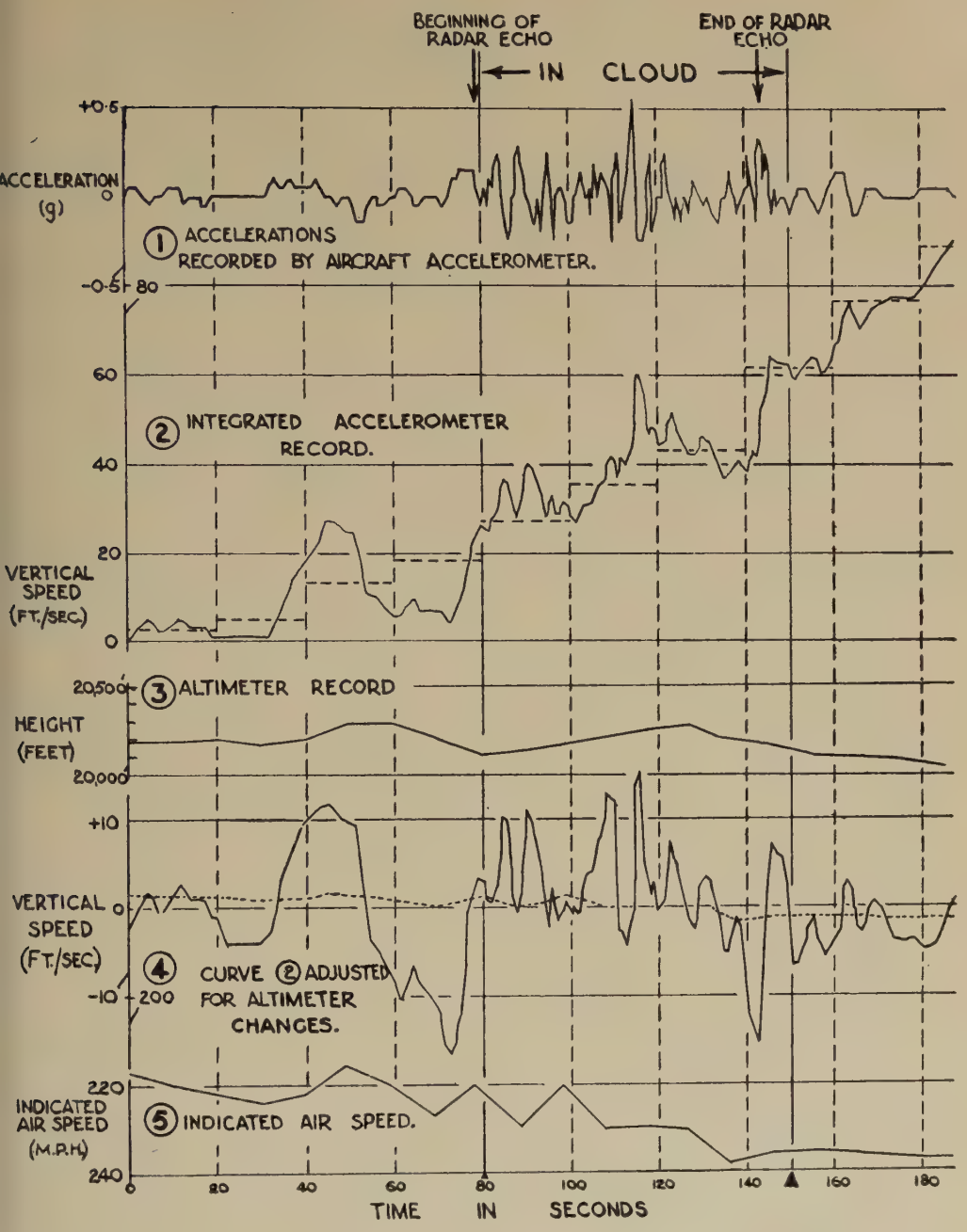


Figure 4. Cumulonimbus investigation – 13 June 1952 – Flight IV at 20,200 ft.

(ii) The *altimeter* readings at 10-sec intervals.

(iii) The *indicated air speed* at 10-sec intervals. The aircraft was trimmed for level flight as soon as it was on course for the flight and therefore some three minutes' flying time away from the cloud. If the power setting and attitude of the aircraft remained unchanged, the indicated air speed would be constant. The power setting was not usually changed during a flight so that changes in indicated air speed would be an indication

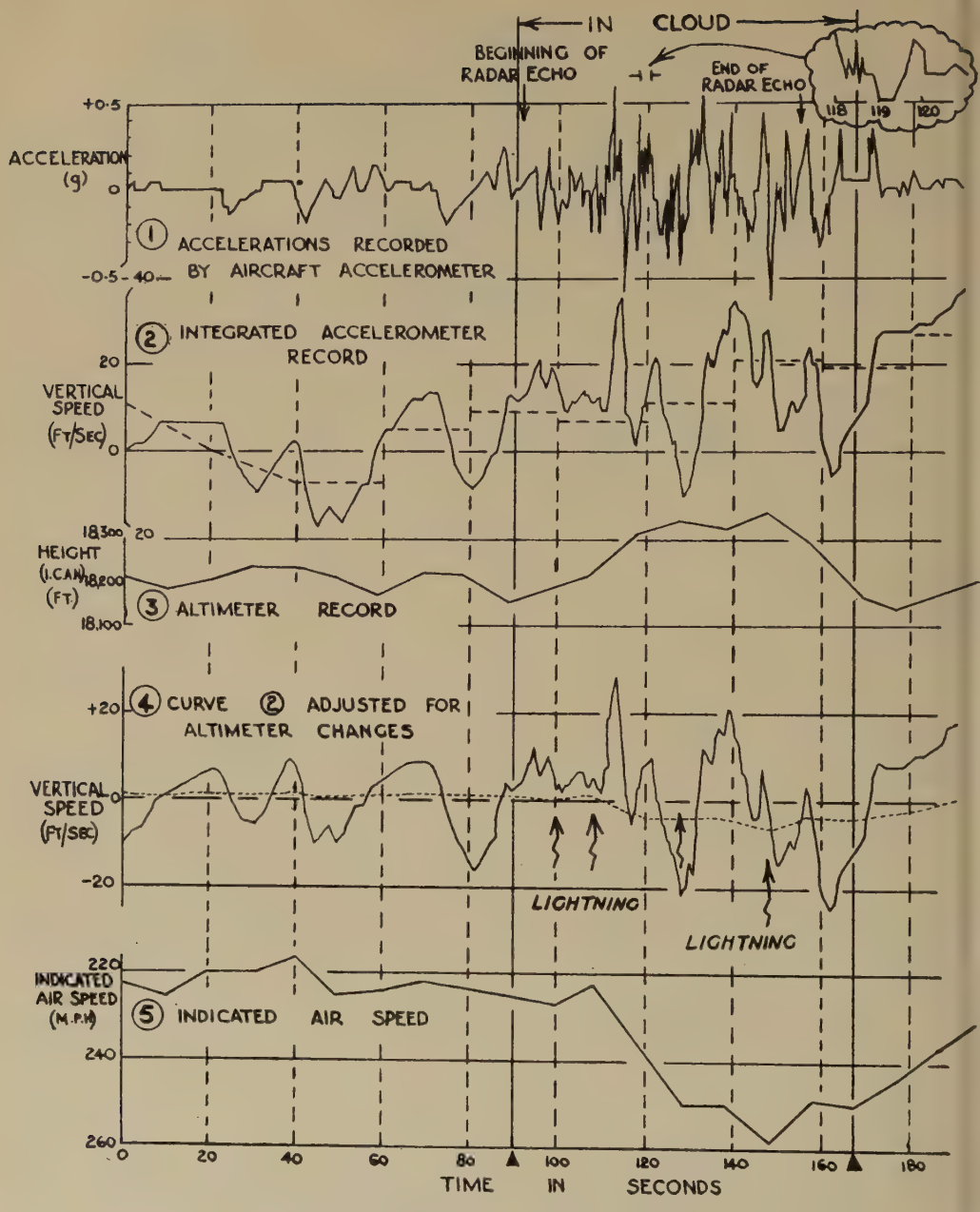


Figure 5. Cumulonimbus investigation – 13 June 1952 – Flight V at 18,200 ft.

of the attitude (climb or dive) of the aircraft. The pilot was briefed to maintain constant attitude, but nevertheless some variations in air speed occurred, indicating that the aircraft was inadvertently in a climbing or diving attitude. The upward or downward movements of the aircraft therefore, as shown by the altimeter or integrated accelerometer curve, were the combined effects of aircraft attitude and upward or downward air currents. A curve showing the rate of climb or descent associated with a change in indicated air speed, for the aircraft used with power setting for 200 kt when in level flight at 20,000 ft, is shown in Fig. 6.

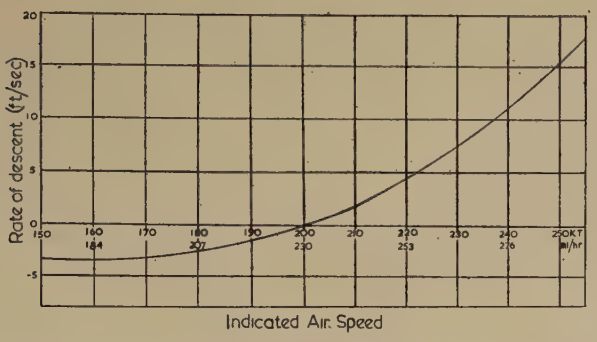


Figure 6. Variation of rate of descent with indicated air speed (power setting constant).

(iv) The *air temperature* at 10-sec intervals. The thermometer used was a ratiometer type temperature indicator – Sangamo-Weston Type S110, Form 9. The temperature element is a platinum wire wound on a conical head and enclosed in hydrogen. The speed corrections had been carefully determined and it is considered that the readings, when outside cloud, were correct to ·25°C with a lag of about $1\frac{1}{2}$ sec. Uncertainty of the corrections to be applied inside cloud, however (Meteorological Office 1945) greatly limits the usefulness of the readings and the detailed traces are not exhibited.

(v) A *tape recording* of the comments made by the observer in the aircraft during a cloud traverse and received from the aircraft on R/T. This includes notes of times of switching the accelerometer on and off, and times of entry into and exit from cloud.

(vi) *Radar cross-sections* in the vertical of the storm echo along the line of flight of the aircraft, taken on wavelengths of 10 cm and 3 cm (beam widths to half power 1·5° and 0·7° respectively in the vertical plane, and 7·5° and 2° respectively in the horizontal plane). An average of eight photographs is available for each flight. On many of these the aircraft echo is also visible, thus enabling the position of the aircraft relative to the echoing volume of the clouds to be known accurately at any instant.

4. Presentation of the data

The data for each of the five flights are shown separately in Figs. 1 to 5 respectively.

In each figure the first graph shows the accelerations experienced by the aircraft; the second is the integration of the acceleration curve, giving a curve of vertical speed against time. The third graph shows the altimeter readings at 10-sec intervals. The altimeter changes in a given time should be related to the integral of the second graph over the same period of time and the dotted lines on the second graph are the zero corrections which must be applied to give the correct altimeter changes. The fourth graph shows the second curve after adjustment to agree with the altimeter changes. The transition from one zero correction to the next has been smoothed, taking account of the form of the original integrated curve. This fourth curve should give a true representation of the upward and downward speeds of movement of the aircraft. The fifth graph gives the indicated air speed at 10-sec intervals.

The parts of the records obtained within cloud and the instants of entry into and departure from the echoing volume associated with the cloud are also shown. On Figs. 2 and 5, graph (4), the places where lightning was reported by the observer in the aircraft are indicated.

5. Evaluation of upward and downward air currents

The interest of the integrated curves lies in the deductions which can be made from the behaviour of the aircraft about the air currents encountered by the aircraft. If an aircraft, flying level, enters an upward air current and maintains its attitude it will ultimately take up the upward speed of the air, provided the air current has a large enough horizontal extent. The time taken to do this is of the order of 1 sec for the aircraft used. Provided the aircraft was flying level therefore (i.e., in these cases, if the power setting was constant, provided the indicated air speed was constant) it can be inferred that the upward air speeds were at least as great as the upward speeds of the aircraft, but were not much greater if the upcurrent took more than a second or two to cross.

Unfortunately on many occasions, presumably because of the turbulence encountered, the indicated air speed did not remain constant. From Fig. 6, however, it will be seen that the changes in upward or downward speed of the aircraft, occasioned by changes in attitude, were usually small, and dotted lines indicate the corrections to be applied to curve (4) of each figure, to account for the variations shown in curve (5). By using these dotted lines as lines of zero vertical speed, curve (4) will then give a reasonably accurate presentation of the vertical speeds imposed on the aircraft by the up or down air currents and hence of the speeds of the air currents themselves.

6. Discussion of results

(a) *Turbulence*

The most striking feature of Figs. 1 to 5 is the marked change in frequency and intensity of gusts on entering cloud. In the clear air before entering and after leaving cloud there was only one gust as great as 0·4 *g* in the five flights (at 257 sec on Flight II – see Fig. 2) and this may have been occasioned by an abrupt change in aircraft attitude (see indicated air-speed trace). The frequency of gusts detected in the clear air was, on the average, 1 per 5·3 sec (equivalent to about 2,900 ft) compared with an average frequency in cloud of 1 gust per 1·4 sec (about 770 ft).

All the gusts as great as 0·5 *g* occurred within the part of the cloud from which an echo was received; the frequency of gusts in cloud, but not in the echoing volume, was 1 per 1·9 sec i.e., gusts were slightly less frequent outside the echoing volume, but great significance cannot be attached to the result since only 66 sec of the whole duration of the flights qualified in the determination of this frequency.

(b) *Upward and downward air currents*

The authors of *The Thunderstorm* (Thunderstorm Project 1949) draw a clear distinction (pp. 39, 43) between gusts and ' drafts.' They regard the gusts as caused by eddies superimposed on the general airflow and carried along by it. On this interpretation the vertical movements of the aircraft are regarded as caused by the large-scale air currents and the strength of the air current is deduced from the change of altimeter reading; the gusts recorded are regarded as effects entirely separate, with no permanent effect on the upward and downward movement of the aircraft. While it is very likely that such gusts or eddies do exist, the fact remains that *recorded* gusts are recorded accelerations of the aircraft in the vertical and that, if due account is taken of their magnitude, shape and duration, their effect on the vertical movement of the aircraft can be determined. In fact the integrated effect on all the gusts should give the speed of movement of the aircraft in the vertical. The only exception to this occurs if the aircraft can enter an up or down current without recording a detectable acceleration. Mean upcurrents of 30-40 ft/sec, as determined from the aircraft altimeter trace, were quite commonly

encountered in Florida and Ohio (Thunderstorm Project 1949) and to enter these without recording a detectable acceleration, i.e. without recording a gust, would require a very gradual increase in speed of upcurrent with horizontal distance. A region of upcurrent, of mean value 32 ft/sec, would need to have horizontal dimensions exceeding 24,000 ft for an aircraft travelling at 300 ft/sec to be able to cross it without experiencing a detectable acceleration (0·05 *g*). On the other hand two up-gusts of 1·0 *g*, each lasting for only 1 sec and giving a mean acceleration of 0·5 *g* for that second, would impart to the aircraft an upward speed of 32 ft/sec. Many of the recorded gusts therefore are probably not caused by small eddies superimposed on a major up or downcurrent but are really a demonstration of the strong gradient, along the line of flight, of the vertical speed of the major current itself. It seems most likely therefore that the integrated accelerometer trace (i.e. the integrated gusts) will give all the major upward or downward speeds of the aircraft, however caused, as well as some smaller-scale eddy fluctuations. A convincing demonstration of this is given by curves (2) and (3) of Fig. 2 in which the three major up and downcurrents (after 120 sec) shown by the integrated accelerometer record, even before corrections are applied, have their counterpart in the variation of aircraft height as shown by the altimeter record.

The outstanding feature of the curves of vertical speed is the close proximity of up and downcurrents of comparable magnitude. An extreme example occurs at 200 sec in Fig. 1 when the aircraft changed from a maximum upward speed of 30 ft/sec to a maximum downward speed of 23 ft/sec in 4 sec, a distance of about 750 yd, but even in the wider air currents of Fig. 2, for example, there appears to be a direct transition from upcurrent to downcurrent. A similar feature was to be observed in the integrated traces previously referred to (Jones 1950). Some of these transitions may be brought about by large eddies rather than by up or downcurrents persisting through a great depth of cloud and, while it is unlikely that all are so caused, there is no means of separating large eddies from extensive up or downcurrents. However they are caused, one is forced to concede their existence within a mature cumulonimbus cloud. The danger to a pilot attempting to fly at constant *altitude* is obvious, for to maintain altitude in an upcurrent of 30 ft/sec, say, he must take up a diving attitude sufficient to give a rate of descent in still air of 30 ft/sec. A quick transition to a downcurrent of 30 ft/sec will find him descending at 60 ft/sec and requiring a change to a rate of climb of 60 ft/sec to maintain altitude. Thus the effect of up and downcurrents and the strain on the aircraft are accentuated.

Examination of Figs. 1 to 5 shows that most of the upcurrents on these occasions were about 3,000 to 5,000 ft across (although two were about 10,000 ft on Flight II) and the downcurrents usually 1,500-4,000 ft across. This is broadly in agreement with the experience reported in *The Thunderstorm* (Thunderstorm Project 1949) from a much greater number of occasions. With these small dimensions it is not surprising that flights only a few minutes later, at the same height and on the reciprocal track, are not similar in detail of up and down currents or of turbulence encountered (e.g. Figs. 1 and 2, Figs. 3 and 4).

Outside cloud, up and downcurrents would appear to have been of more variable, and usually greater, width. After allowing for variations in indicated air speed, it would appear that downcurrents approaching 20 ft/sec may exist on some occasions outside cloud but upcurrents of this magnitude were only encountered within the cloud.

(c) *Temperature*

Uncertainties of the corrections to be made to temperatures recorded in cloud – the corrections may vary in different parts of the same cloud according to the water content – limit the usefulness of the readings obtained. The mean temperatures and standard deviations for each height are shown in Table 1.

TABLE 1. TEMPERATURES RECORDED IN CLOUD AND CLEAR AIR

Height (ft ICAN)	Mean temp. in cloud (°C)	Standard deviation (°C)	No. of obs.	Mean temp. clear air (°C)	Standard deviation (°C)	No. of obs.
25,200	− 30·7	0·6	27	− 30·7	0·6	31
20,200	− 19·5	0·9	12	− 19·2	0·7	23
18,200	− 16·2	0·7	7	− 15·2	0·3	12

In this table the effect of variations in height during a flight has been minimised by reducing all readings to a standard height, using a dry adiabatic correction for readings outside cloud and the appropriate wet adiabatic correction inside cloud. As the departures from the mean level of flight were always less than 300 ft the corrections applied on this account were never as great as 1°C.

The differences between the dry- and wet-bulb speed corrections at the speeds flown by the aircraft amount to 3·1°C at 18,000 ft, 2·3°C at 20,000 ft and 0·8°C at 25,000 ft so that temperatures uncorrected for conditions in cloud may be too low, at least in some parts of the cloud, by these quantities. Taking this into account it would seem likely that the mean temperatures in cloud were higher than in the clear air at the same height with the differences in temperature not likely to exceed 2·0°C at 18,200 and 20,200 ft and 0·8°C at 25,200 ft. Applying the full possible corrections the lapse rates in cloud would be 2·1°C/1,000 ft between 18,200 and 20,200 ft and 2·5°C/1,000 ft between 20,200 and 25,200 ft, which are very close to the saturated adiabatic lapse rates (saturation with respect to water) for a wet-bulb potential temperature of about 17°C (2·2°C, 2·4°C and 2·8°C per 1,000 ft at 18,200 ft, 20,200 and 25,200 ft respectively). The lapse rates in clear air, 2·0°C/1,000 ft and 2·3°C/1,000 ft between 18,200 and 20,200 ft and 20,200 and 25,200 ft respectively are less than the saturated adiabatic lapse rate.

It is of interest to note that the variability of temperature within cloud, as evidenced by the standard deviation of the observations, is not significantly greater than outside cloud at 20,200 and 25,200 ft.

(d) *Lightning*

Lightning was reported by the observer in the aircraft by R/T at the time of its occurrence on one occasion on Flight II at 25,200 ft and four occasions on Flight V at 18,200 ft. A time lag of 2 sec has been allowed for the time to report and the points of occurrence are indicated in the appropriate places on curve (4) of Figs. 2 and 5.

It will be seen that on all occasions but one (at 128 sec on Flight V) the aircraft was not experiencing any appreciable upward or downward movement while on the other occasion it was descending at about 20 ft/sec. About 5 ft/sec of this downward speed was ascribable to change in attitude of the aircraft.

The fact that lightning was not reported on Flights III and IV which were only 2,000 ft higher than Flight V and 11 min and 6 min earlier respectively suggests that Flight V was made at almost exactly the time when lightning first occurred within the cloud. From the radar-echo photographs it was clear that the cloud had already reached its maximum vertical extent by the time Flight III was made.

(e) *Correlation with radar echo*

The limitations of the 10-cm wavelength radar, used in previous analyses of cumulonimbus flights, to determine correlations between radar echo and turbulence, have been

discussed (Jones 1954). During the flights here described, a 3-cm wavelength height-range radar was also used which gave greater precision because of its narrower beam width (0·7° in the vertical and 2° in the horizontal plane). It was found that even the small change of bearing necessary to keep the aircraft in the radar beam can substantially change the radar picture. This makes correlation with the radar echo more difficult but it is certain that all gusts of 0·5 g or greater were encountered within portions of the cloud giving rise to the radar echo and were often, as reported previously (Jones 1954), associated with discontinuities in the echo structure. There was some evidence that upcurrents and echo columns coincided while a substantial downcurrent would seem to have existed outside the edge of the echoing volume but still within cloud on Flight V and just outside echoing volume and cloud on Flights II and IV.

On Flights I and III the echoing volume began about 12 sec (about $1\frac{1}{4}$ mi) before the observer reported entering cloud, suggesting the existence of precipitation from overhanging cloud. On Flights IV and V the edge of the echoing volume and cloud edge coincided while on Flight II the aircraft was in cloud about 12 sec (about $1\frac{1}{4}$ mi) before entering the echoing volume. On the other hand the aircraft always left the echoing volume before leaving cloud by times varying between 7 and 17 sec ($\frac{3}{4}$ to $1\frac{3}{4}$ mi).

7. Conclusions

(i) It is possible to obtain reliably from an accelerometer trace and altimeter record the detailed movements of an aircraft in the vertical and thence, if indicated air speed is also recorded, to deduce the strengths of the upward and downward air currents.

(ii) The form of the integrated curve, alone, suggests the corrections to be applied and useful information could be obtained even if altimeter readings were not available.

(iii) Upward and downward air currents of comparable magnitude, which may often exceed 20 ft/sec, occur close to each other in mature cumulonimbus clouds.

(iv) Downcurrents approaching 20 ft/sec may occur outside cloud but upcurrents of this magnitude were all recorded within cloud.

(v) There is a striking change in frequency and intensity of gusts on entry into cloud and the biggest gusts all occur within the echoing volume of the clouds.

(vi) Mean temperatures in cloud, although difficult to measure with certainty, were probably up to 2°C higher than the mean temperature of the environment at the same height, at 18,200 and 20,200 ft, and up to 0·8°C higher at 25,200 ft.

(vii) Lightning was observed once at 25,200 ft and four times at 18,200 ft, and on all but one occasion the aircraft was probably not in any appreciable up or downcurrent. The cloud had already attained its maximum vertical development before lightning was reported.

Acknowledgments

Grateful acknowledgment is made to Flt. Lt. Eccleston, R.A.F. and Flt. Lt. R. Allen, R.N.Z.A.F., the pilot and observer on the aircraft during these flights and to Mr. A. W. E. Barber, Meteorological Office, East Hill for drawing Figs. 1 to 5.

I am indebted to the Director of the Meteorological Office for permission to publish this paper.

References

Jones, R. F. 1950 *Air Ministry, Met. Res. Ctee., M.R.P.*, No. 593.
1954 *Prof. Notes, Met. Off.*, London, No. 109.

Meteorological Office 1945 *Meteorological Air Observer's Handbook*, London, Air Ministry, M.O. 470.

Thunderstorm Project 1949 *The Thunderstorm*, U.S. Government Printing Office, Washington, D.C.

551.578.11

Modification of the size distribution of falling raindrops by coalescence

By B. J. MASON and R. RAMANADHAM
Imperial College, London

(Manuscript received 14 February 1954

Summary

The manner in which the size distribution of raindrops is modified during their fall by mutual coalescence, accretion with cloud droplets, and by evaporation below cloud base is investigated. It is shown that if a constant flux of raindrops having an exponential size distribution occurs at the level of origin (just below the melting level in a layer-cloud) to give a steady-state, this initial distribution will undergo considerable modification after a fall of 1 km, the smaller drops being seriously depleted and the larger ones increased. All three processes will cause a shift in the drop-size spectrum towards larger diameters and produce an increase in the intensity of the radar echo at lower levels of the kind which is sometimes observed. Because of this modification of the drop-size spectrum during fall and because of the rather rapid fluctuations which are frequently observed in the distribution at the ground, it is not often possible to correlate the latter with the intensity of the radar echo located higher up.

1. Introduction

It has been proposed by Marshall and Palmer (1948) that the size distribution of raindrops reaching the ground may be represented by an expression of the form

$$N_D = 8 \times 10^{-2}\, e^{-xD}\ \text{cm}^{-4}, \quad x = 41\, R^{-0\cdot21}\ \text{cm}^{-1} \qquad . \qquad . \qquad (1)$$

where $N_D\, \delta D$ is the number of drops per unit volume of diameters between D and $D + \delta D$, and R is the rate of rainfall in mm hr^{-1}. This equation was found to provide a good *average* fit to the experimental data of Laws and Parsons (1943) obtained from many drop samples, apart from a tendency to overestimate the numbers of small droplets, when $xD < 4{\cdot}5$. The present authors have also found that the drop-size data obtained with their photoelectric spectrometer (Mason and Ramanadham 1953) can be represented *on the average* by Eq. (1), but that a sample taken over a period of several minutes on any one occasion may show considerable deviations from this exponential distribution, with peaks often appearing in the spectra as shown in Fig. 1. Moreover, even in so-called ' steady,' warm-frontal rain, the drop-size distribution often fluctuates appreciably from minute to minute.

In seeking an explanation for these observed variations in the drop-size distribution about a roughly exponential mean distribution, we have investigated the following factors which may modify it while the rain falls from its level(s) of origin to the ground : (a) coalescences between raindrops of different sizes; (b) the growth of raindrops by accretion with cloud droplets; (c) differential rates of evaporation of drops of different sizes when falling between cloud base and ground. It is evident *a priori* that all three processes will tend to shift the spectrum towards larger drop-sizes, while (a) and (c) will cause a depletion in the numbers of smaller drops.

An examination of these matters has been made by Rigby and Marshall (1952) who concluded that these processes cannot be of major importance in determining the form of the drop-size spectrum. Our more detailed calculations show that an exponential distribution of drops originating at say, the melting level in a stratiform cloud, may be considerably modified by these processes during their journey towards the ground, and that if a Marshall-Palmer distribution is observed at the ground it must originate from a considerably steeper distribution at the melting level. If the fall-path is greater than about a kilometre the changes should be detectable by radar.

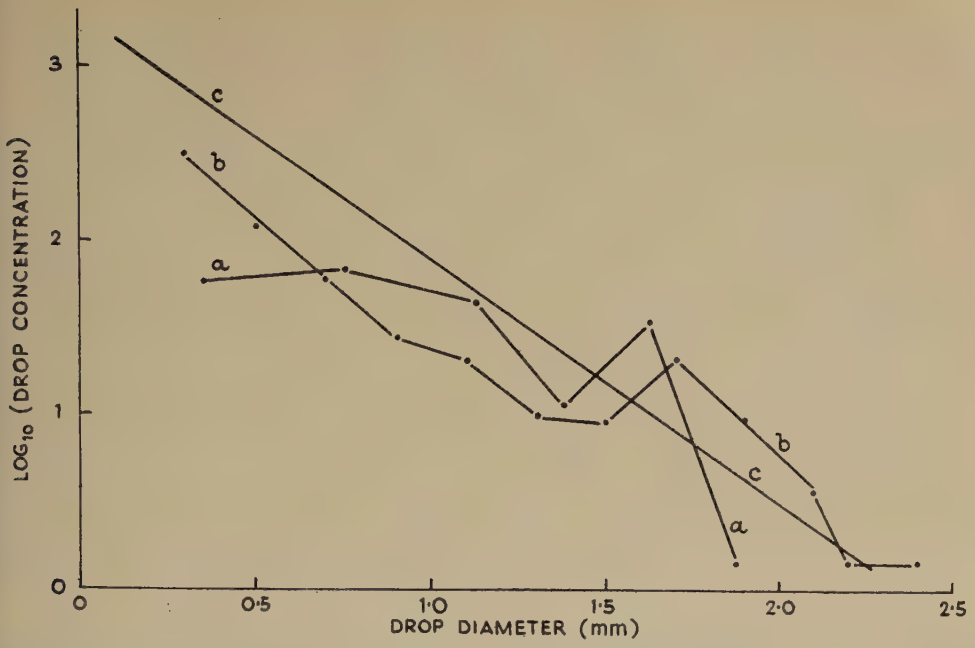


Figure 1. Drop-size distributions observed at the ground with rainfall intensities of 3 mm hr^{-1}. (a) Mason and Ramanadham with photoelectric spectrometer, (b) Blanchard (1953) with filter-paper method from a cloud with top above freezing level in Hawaii, (c) the Marshall-Palmer distribution.

2. Modification of the drop-size spectrum by coalescence of raindrops

We shall assume for the purposes of calculation that when ' steady ' rain is falling from a layer-type cloud, a constant flux of raindrops having an exponential size distribution passes through a horizontal surface just below the melting level. We then calculate how this initial distribution is altered with distance fallen, by the larger drops overtaking, colliding, and presumably coalescing with smaller raindrops. Since we assume a constant generation of drops at the melting level, the flux of rain water (the rainfall intensity R) must be the same at all levels, and since the drop-size distribution curve is displaced towards larger diameters at lower levels, the total concentration of drops must concurrently decrease. Such a steady-state can be attained only after the smallest drops produced at the level of origin have reached the ground – in a typical case this may be several minutes after the largest drops arrive.

Let us consider two groups of raindrops of diameters D, D_1, $(D > D_1)$, spatial concentrations N, N_1, and fall-speeds v, v_1, passing through the level of origin. After falling through a distance z in time z/v, the larger drops will have collided with smaller drops of earlier origin in a column of length $(v - v_1)\, z/v$. The number of drops of diameter D_1 removed per second from unit volume of a column of unit cross-section will be $\delta N_1/\delta t = \frac{\pi}{4}(D + D_1)^2\, N\, N_1\, (v - v_1)$, $z\,\frac{\delta N_1}{\delta t}$ being the difference $\Delta N_1\, v_1$ between the numbers of the drops entering the top of the column and leaving the bottom of the column per second. Hence,

$$\Delta N_1 = \frac{\pi}{4}(D + D_1)^2\, N\, N_1\, \frac{v - v_1}{v_1}\, z \qquad . \qquad . \qquad . \qquad (2)$$

Eq. (2) which was originally derived by Rigby and Marshall, can be used to calculate the number ΔN_1 of drops of diameter D_1 removed from unit volume by larger drops of diameter D and concentration N after falling through a distance z.

In making the computation it is important to realize that all larger drops are competing for the D_1 drops and that N_1 in Eq. (2) cannot be treated as constant unless the number of collisions is small compared with N_1 itself; it is therefore necessary to calculate ΔN_1 for small intervals of z to ensure that this is the case and to adjust the value of N_1 for successive z-intervals. It is not permissible, particularly when considering the removal of the smaller drops, to assume N_1 to remain constant during a fall-path of several hundred metres as did Rigby and Marshall who, in effect, considered only the growth of a single particle and ignored the effects which arise from competition between the larger particles for the smaller ones. Furthermore, the values of N and D in Eq. (2) are also changing continually as the result of coalescences, so that they can be assumed constant only during a restricted time interval. We have, in fact, a multi-particle problem whose solution can be obtained only by laborious step-wise computation. We have carried out one such calculation, the results of which are summarized in Table 1 and plotted in Fig. 2.

Starting with an assumed exponential size distribution of drops at the melting level, 1 km above cloud base, we have computed the number of drops in each size group removed by all larger drops; e.g., the figure in bold type shows that from an original concentration of $3{,}200/m^3$ of drops with diameter 0·02 cm, $94{\cdot}7/m^3$ are removed by drops of diameter 0·12 cm originally present in a concentration of $10{\cdot}1/m^3$. Addition of each vertical column gives the total numbers of drops of each size which must be removed from unit volume to give their concentrations at the cloud base ($z = 1$ km), but each of these columns is the result of a step-wise computation using small intervals of z, in which N_1 of Eq. (2) was adjusted at each step.

It is also necessary to remember that while drops in a given size group decrease in number because of capture by larger drops, at the same time, some of them increase in diameter by capturing smaller drops. The horizontal columns in Table 1 show how many smaller drops of each size are removed from unit volume by drops in a given group. In calculating the increase in diameter of the drops in a given size group due to capture of smaller drops, it is not realistic to determine the *average* diameter increment for each group as did Rigby and Marshall, because the number of drops captured by a particular group may be considerably less than the number in the group itself. Table 1 shows this to be so in just those cases (large values of D_1) where the increment in diameter due to coalescence would be largest. We have accordingly calculated the diameter increases for the drops in each group in terms of the probabilities of coalescences with smaller drops of each size. For example, Table 1 shows that 569 drops of average initial diameter 0·13 cm (i.e. lying in the range 0·125 to 0·135 cm) remove 464 drops of diameter 0·06 cm, 217 of diameter 0·07 cm, 98 of diameter 0·08 cm, etc. We have calculated how many of the 569 drops, on average, remove both a drop of diameter ·06 cm and one of ·07 cm, i.e. (·06 + ·07), how many remove (·06 + ·08), (·06 + ·09) . . . (·07 + ·08), (·07 + ·09) . . . etc. In this way, the diameter increments of all the 569 drops of original diameter 0·13 cm are determined, and similar calculations are made for drops of all sizes. It is found that as a result of growth by coalescence, some drops are shifted into higher size groups, but in consequence of this and their consequently increased fall velocities, their concentrations have to be continually adjusted during the calculation in order to keep the flux of rainwater the same at each level.

In Table 1, line (a) shows the assumed initial distribution at the melting level, and line (b) the distribution which results from coalescences between raindrops during their fall to the cloud base, 1 km below. The choice of the initial size distribution, which is different from that of Marshall and Palmer, calls for comment. Preliminary calculations indicated that if a distribution similar to that of Eq. (1) were to be found at the ground, the effects of coalescence and evaporation would require the distribution at the melting

TABLE 1. MODIFICATION OF DROP-SIZE SPECTRUM BY COALESCENCES OF RAINDROPS

Initial size distribution at melting level (0°C, 800 mb), $R = 3$ mm hr^{-1}

D (cm)	0·01	0·02	0·03	0·04	0·05	0·06	0·07	0·08	0·09	0·10	0·11	0·12	0·13	0·14	0·15	0·16	0·17
(a) $N_D\,\delta D$ (per m^3)	5690	3200	1820	1010	569	320	182	101	56·9	32	18·2	10·1	5·69	3·20	1·82	1·01	0·569
v (cm/sec)	27	76	125	172	217	260	302	344	386	426	462	493	523	551	578	601	623

Numbers of drops of each size removed by larger drops

D/D_1	0·01	0·02	0·03	0·04	0·05	0·06	0·07	0·08	0·09	0·10	0·11	0·12	0·13	0·14	0·15	0·16	0·17
0·02	280·9																
0·03	567·5	256·1															
0·04	726·4	396·3	147·2														
0·05	773·8	447·5	211·9	67·5													
0·06	726·4	432·9	222·1	91·8	27·8												
0·07	629·6	375·7	196·3	93·2	37·3	10·97											
0·08	542·0	310·5	170·6	81·6	35·9	14·25	4·15										
0·09	402·3	243·0	136·4	66·7	31·7	13·70	5·27	1·56									
0·10	309·2	184·2	103·3	51·7	25·0	11·51	5·02	1·93	0·468								
0·11	224·7	133·5	76·0	38·6	18·9	8·99	4·02	1·70	0·612	0·171							
0·12	100·7	94·7	52·9	27·1	13·6	6·56	3·08	1·39	0·640	0·191	0·050						
0·13	101·5	65·2	36·7	18·7	9·4	4·64	2·17	0·98	0·429	0·171	0·059	0·025					
0·14	71·8	44·1	24·9	12·7	6·4	3·14	1·40	0·68	0·284	0·130	0·050	0·025	0·010				
0·15	40·4	29·6	16·8	8·7	4·4	2·15	1·02	0·49	0·228	0·098	0·040	0·025	0·010				
0·16	34·0	19·8	11·0	5·6	2·8	1·47	0·65	0·39	0·156	0·070	0·030	0·012	0·006				
0·17	22·1	12·5	7·2	3·7	1·9	0·99	0·47	0·20	0·106	0·051	0·020	0·012	0·006				
	5613	3046	1413	568	215	78·4	27·3	9·23	2·82	0·882	0·249	0·099	0·032				
(b) $N_D\,\delta D$ at cloud base (coalescence of raindrops)	77	154	407	380	302	265	187	99·7	65·2	36·9	21·2	14·3	7·2	4·50	2·82	2·25	0·96	0·59
(c) $N_D\,\delta D$ at cloud base corrected for accretion			204	420	395	313	232	166	98	58	34·2	20	12·8	7·0	4·15	2·49	1·48	0·87
(d) $N_D\,\delta D$ at ground			27	120·9	172·1	175·2	155	105	91	58	42·6	24·5	18·6	11·6	8·0	5·5	3·7	2·5
(e) $N_D\,\delta D$ at ground corrected for evaporation	24·5	80·5	118·4	138·3	142·5	126	105	82	62·5	46·2	31·7	21·0	14·8	10·3	7·0	4·9	3·3	2·3

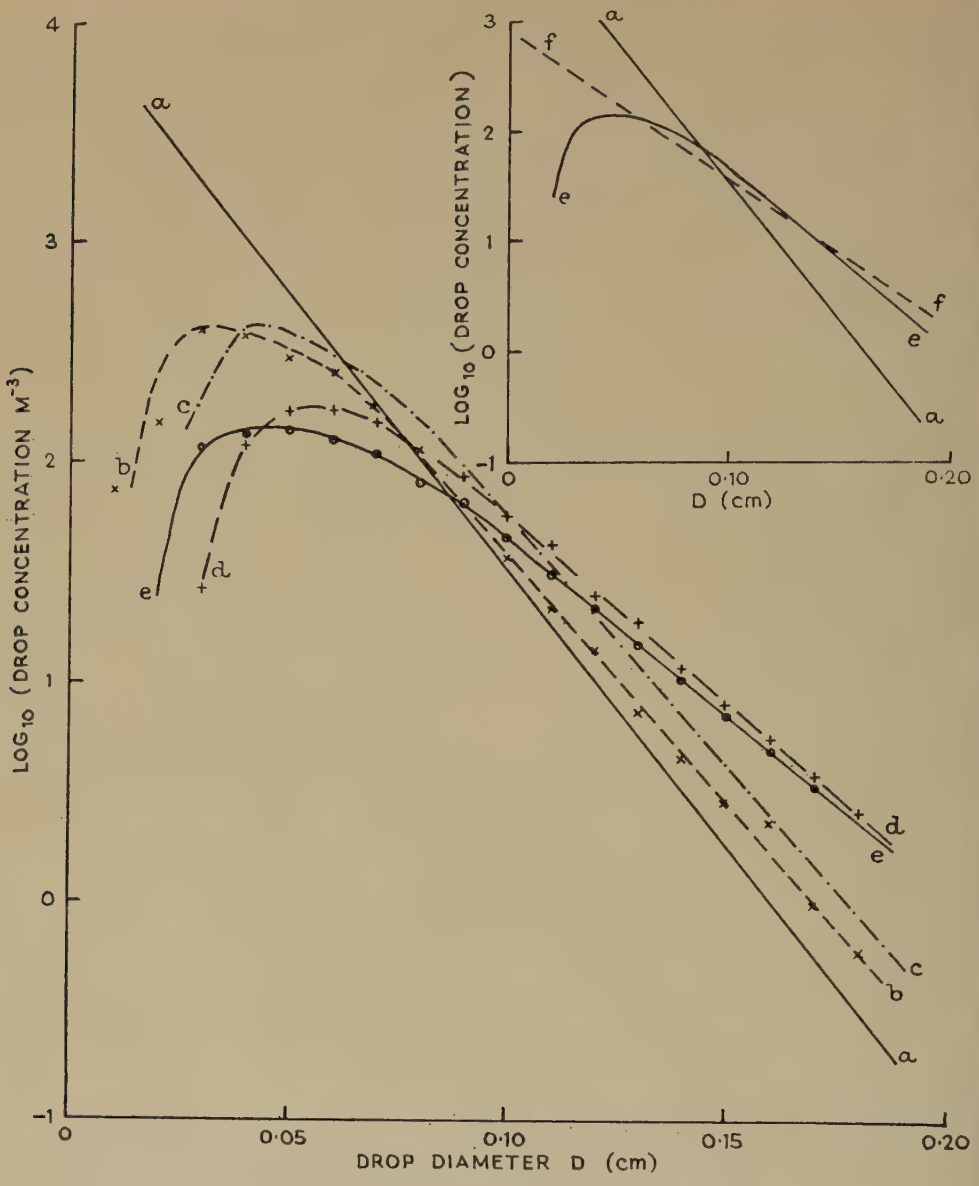


Figure 2. Modification of size distribution of raindrops by coalescences and evaporation.

level, 2 km higher up to be considerably steeper. We therefore made a choice of the numerical factors in (1) to achieve a steeper distribution corresponding to a rainfall intensity of 3 mm hr^{-1}, the drop-diameter spectrum ranging from ·01 to 0·2 cm. Line (c) of Table 1 shows the distribution at cloud base when the growth of the raindrops by accretion with cloud droplets (see next section) is taken into account. This new distribution is now taken as the starting point of a new computation to determine the size distribution at the ground (20°C, 1,000 mb), after a further fall of 1 km. During this last stage, we have to consider modification of the spectrum by coalescences between raindrops (line (d)) and by evaporation of the drop falling in through unsaturated air (line (e)). The calculated size distributions at the cloud base and at the ground, together with the assumed initial exponential distribution are plotted in Fig. 2; inset are shown

the final distribution at the ground, curve (e), the Marshall-Palmer distribution appropriate to this rainfall intensity (3·63 mm hr^{-1}), line (f), and the original distribution at the melting level, line (a). It will be seen that the effects of coalescence and evaporation cause drops of diameter less than 0·06 cm to be seriously depleted, while there is a corresponding increase in drops of diameter greater than 1 mm. The overall result is to produce a marked flattening of the original distribution, so that the distribution at the ground is much closer to that of Marshall-Palmer for the corresponding precipitation intensity than to the original distribution at the melting level.

3. The effects of accretion of cloud droplets by raindrops

A raindrop, after falling through a depth z of cloud, the cloud-water content of which is w, will increase in diameter by

$$\Delta D = Ewz/2\rho \quad . \quad . \quad . \quad . \quad (3)$$

where E is the collection efficiency of the raindrop for the cloud droplets and ρ is the density of the raindrop. Values of E have been calculated by Langmuir (1948) who, however, neglected the finite size and fall-velocity of the smaller drops. Adjusted values in which these factors, have been taken into account have been computed; the cloud-droplet radius is taken as 8 μ, the temperature 0°C and the pressure 900 mb. In calculating the shift in the raindrop-size spectrum caused by accretion, we have assumed a cloud-water content of 0·2 g^{-3}, so that after falling a distance of 1 km the drop diameter increases by $\Delta D = 10^{-2} E$ cm. The results are shown in curve (c) of Fig. 2.

The effect of this accretion process on the raindrop spectrum is to cause a shift of nearly ΔD towards larger drop sizes, but it does not influence appreciably the general shape of the curve which is much more affected by the coalescence of raindrops. The growth of the raindrops by sweeping up the cloud droplets does, however, cause the rainfall intensity to increase from the initial value of 3 mm hr^{-1} to 4·25 mm hr^{-1} at cloud base; this is later reduced by evaporation to 3·63 mm hr^{-1} at the ground in our calculation.

4. Evaporation of raindrops between cloud base and ground

In falling between the cloud base and the ground, the raindrops will tend to evaporate if the relative humidity in the intervening layer is less than 100 per cent. If continuous rain has been reaching the ground for some minutes, it is unlikely that, in general, the humidity in the sub-cloud layer will be much below saturation and almost certainly not below 90 per cent. In this case, drops of diameter greater than 1 mm will shrink by only a few per cent in falling 1 km but drops of initial diameter less than 0·3 mm will evaporate completely. The modification of our raindrop spectrum by evaporation is shown in Fig. 2 (e). The calculations are based on the rates of evaporation obtained experimentally by Kinzer and Gunn (1951) and assuming an atmosphere of 90 per cent humidity and mean temperature 15°C. The effect of evaporation is to reduce the rainfall intensity from 4·25 mm hr^{-1} at cloud base to 3·63 mm hr^{-1} at the ground in the case of our specimen calculation.

5. The radar echo intensities at different levels

The radar echo intensity, $Z \propto \Sigma N_D D^6$, will increase at lower levels as a result of the shift in the raindrop spectrum towards larger sizes and also because of the growth of the drops by accretion. According to the calculations of Table 1, the radar echo

intensities just below the melting level, at the cloud base and at the ground would be in the ratios of 1 : 1·86 : 2·2; of the 86 per cent increase at cloud base, 26 per cent is accounted for by coalescence of raindrops and 60 per cent by the accretion of cloud droplets.

We have observed on the 'A' scope of a vertically directed 3-cm radar that the echo intensity below the melting band sometimes increases slowly with decreasing height. On one occasion, with a rainfall intensity of 3 mm hr^{-1} the echo intensity (corrected for range) increased by 50 per cent over a fall-path of 1,500 m below the melting band.

6. Conclusions

The above calculations show that the drop-size distribution of raindrops may be appreciably modified during their fall by coalescences between raindrops, accretion of cloud droplets and evaporation of the drops below cloud base. An exponential Marshall-Palmer distribution at the ground would originate from a considerably steeper distribution at the level of origin, say, 2 km higher. Fig. 2 shows that although the processes considered above can account for a considerable depletion of the numbers of drops of diameter less than 0·05 cm and some increase in the concentrations of larger drops, the irregularities in the spectra which are observed at the ground (Fig. 1) cannot be accounted for in terms of modification of a steady-state exponential distribution by coalescence and evaporation; statistical fluctuations in the numbers of collisions may result in small variations about the distributions shown in Fig. 2, but could hardly account for the large irregularities of Fig. 1. It appears that even in so-called ' steady ' rain a steady state such as has been assumed in the above calculations rarely occurs.

Because the drop-size distribution at the ground may, on any given occasion, deviate considerably from a Marshall-Palmer distribution, and because the distribution may be appreciably modified by coalescence and evaporation of drops, calculations of the radar echo intensity in terms of a Marshall-Palmer (or any other mean distribution) appropriate to the rainfall intensity observed at the ground, may be very inaccurate. Indeed, quite large discrepancies between experimentally measured values and values calculated on this basis have been found by Austin (1951). Even if simultaneous measurements of the drop-size distribution at the ground are made, and this is changing rather rapidly with time (as is often the case) it is necessary to remember that the drops responsible for the radar echo at any given instant will not reach the ground until some time later and that the distribution will be changed during their journey. In such studies it would be highly desirable to measure the drop-size distribution in the echoing region and to use a radar with very short minimum range.

Acknowledgment

One of us (R. R.) wishes to express his thanks to Andhra University for granting him leave of absence to carry out this work and for the award of a research scholarship.

References

Austin, P. M.	1951	*Proc. Illinois Conf. on Water Resources*, p. 227.
Blanchard, D. C.	1953	*J. Met.*, **10**, p. 457.
Kinzer, G. D. and Gunn, R.	1951	*Ibid.*, **8**, p. 71.
Langmuir, I.	1948	*Ibid.*, **5**, p. 175.
Laws, J. O. and Parsons, D. A.	1943	*Trans. Am. Geophys. Un.*, **24**, p. 452.
Marshall, J. S. and Palmer, W. McK.	1948	*J. Met.*, **5**, p. 165.
Mason, B. J. and Ramanadham, R.	1953	*Quart. J. R. Met. Soc.*, **79**, p. 490.
Rigby, E. C. and Marshall, J. S.	1951	' Stormy Weather,' *Res. Rep. McGill Univ.*, MW-3.

551.543 : 551.553.532.59

Short-period variations in surface pressure and wind

By I. J. W. POTHECARY
Meteorological Office, Dunstable

(Manuscript received 28 September 1953, in revised form 5 May 1954)

SUMMARY

Periodic variations in surface pressure and wind velocity recorded by autographic instruments on 5 July 1952 are related to wave motion along a horizontal temperature discontinuity. The wave motion is considered to have been set up when the outflow of cold air from an intense outbreak of thunderstorms over the western English Channel temporarily blocked the north-easterly airstream beneath an inversion over southern England and the Midlands.

The oscillation was propagated upstream of the lower current and across the upper south-easterly current for over 250 mi, with an amplitude approaching 200 m in the earlier stages.

1. INTRODUCTION

In the early hours of the morning of 5 July 1952 several stations in southern England recorded an unusual oscillation of wind velocity, and many stations recorded an oscillation of pressure.

There was little apparent change of weather associated with this disturbance, although 24 hours later a thundery outbreak, which was severe in the west of the country, moved northward from the English Channel and was responsible for a marked pressure jump resulting from the normal thunderstorm processes.

An analysis of pressure and wind records, from a number of widely scattered stations in southern England and the Midlands, showed that the oscillation moved north-east against the lower airstream for over 250 mi, at a speed which slowly decreased from an initial value of over 20 m/sec as the amplitude of the effects became smaller.

From the available evidence it is suggested that the effects shown on the autographic records were due to an oscillation set up in an inversion at about 830 mb. A warm layer was overlying a north-easterly airstream and formed the lower boundary of an upper south-easterly current.

It is possible that the oscillation was set up when the lower airstream was temporarily blocked by the outflow of cold air from an intense outbreak of thunderstorms over the western English Channel.

2. SYNOPTIC SITUATION

During 2 July a cold front, associated with a small depression moving north-east along the Norwegian coast, crossed the British Isles and moved into France and the Low Countries, setting off thundery activity over the Continent.

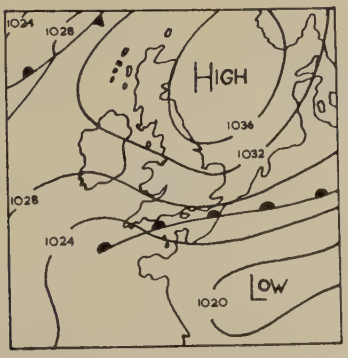


Figure 1. Surface chart for 0001 GMT, 5 July 1952.

By 12 hr, 3 July, the front had become slow moving, and later became retrograde over northern France. Meanwhile an anticyclone moved quickly north-east across the British Isles towards Scandinavia, where it was centred on 5 July.

Shallow thundery lows had dominated the weather over the whole of France since the beginning of the month, and by 24 hr, 4 July, thundery outbreaks had extended across the whole of the Bay of Biscay and the southern half of the English Channel, to the south of the warm front, which, by then, was moving slowly northward.

An extensive area of severe thunderstorms was set off during the night of 4-5 July in the vicinity of the Channel Islands, and it was this thundery area which moved northward across the British Isles during 6 July, with severe thunderstorms in the west.

3. The temperature and wind structure over southern and central England

During the night of 4-5 July surface winds were north-easterly, becoming easterly over Devon, Cornwall and west Wales. Surface windspeeds varied from 5 to 20 kt according to the exposure of the station.

The Camborne, Larkhill, Hemsby and Liverpool radiosonde ascents at 03 GMT, 5 July, showed that a horizontal warm layer, some 150 mb thick, extended over southern England and the Midlands, between 950 and 800 mb. At Larkhill, and to a lesser extent at Camborne, the warm air was present in two distinct layers; one at 950 mb and the other at 830 mb.

The lower layer resulted from night cooling at the surface after a day of convection on 4 July, which gave rise to an approximately dry adiabatic lapse rate below the warm layer already in existence at 830 mb.

The origin of the upper warm layer lay in the intense heating during the previous three days over the Continent and, on 1 July, over southern England, with surface temperatures in excess of 90°F, which produced a layer of convectively mixed air from the surface to 700 mb, to the south of the cold front.

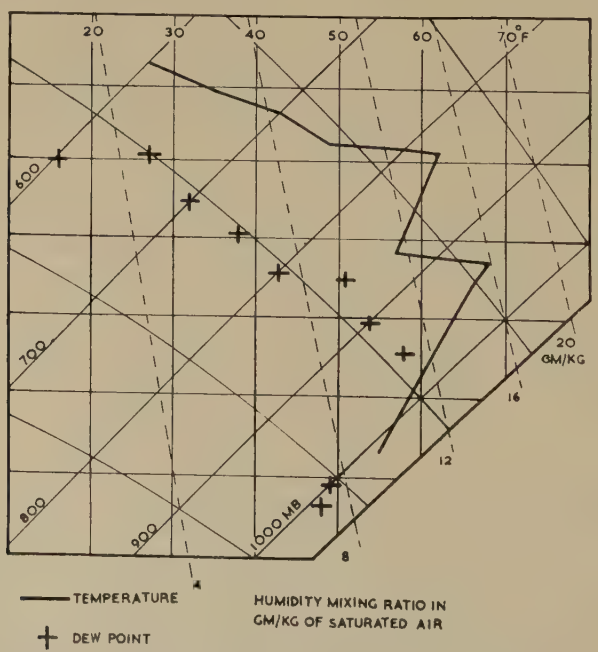


Figure 2. Larkhill radiosonde ascent, 03 GMT, 5 July 1952.

On 2 July the south-eastward movement of the cold front brought cooler air across the British Isles. During 4 July the wind above 900 mb over southern England veered from north-easterly to south-easterly as the front became retrograde, and a layer of warm air spread northward over southern England and the Midlands.

At 03 hr, 5 July, over the Midlands and south-east England, the wind was east-north-east up to 850 mb, with a mean speed of about 20 kt through the layer, veering to south-east with a marked decrease in speed through the warm air above this level. Although the evidence does not indicate the existence of sharp discontinuities, the rate of change of velocity through the inversion is large so that the conditions approximate to an idealized model of the atmosphere in which a discontinuity of velocity coincides with a discontinuity of density. This model is inferred as the lack of wind data from Larkhill at 03 hr precludes any more definite statement of the wind structure.

4. The effect of thunderstorms over the western English Channel

Between 18 GMT and 22 GMT, 4 July, a severe outbreak of thunderstorms occurred over the Channel Islands and the western English Channel. Lightning was reported at midnight to the south-east of Plymouth and south-west of Calshot. A thunderstorm was reported at Portland Bill. Severe thunderstorms were affecting Brittany and the Cherbourg peninsula.

In an area of thunderstorms of this magnitude the outflow of cold air at the surface is considerable and will cause a sudden rise of pressure. This feature was recorded at many places in southern England and the Midlands as thunderstorms moved northward during the morning of 6 July.

It is suggested that a sudden and considerable outflow of cold air from the thunderstorm area abruptly blocked the easterly flow beneath the warm layer somewhere over

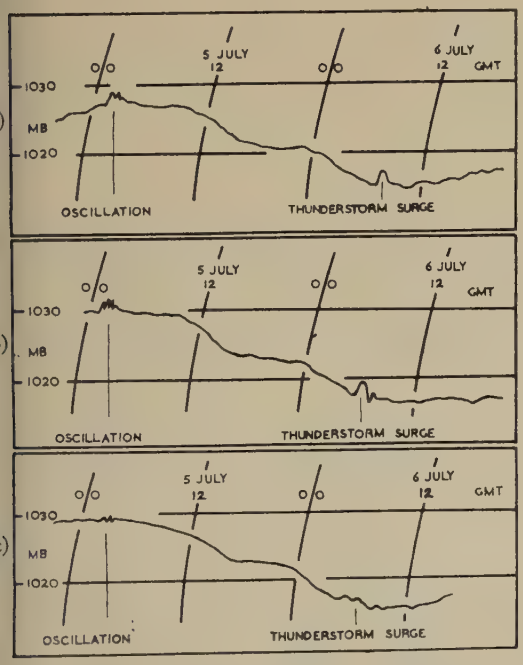


Figure 3. Barograms, 4 July-6 July 1952.
(a) Farnborough. (b) Abingdon. (c) Croydon.

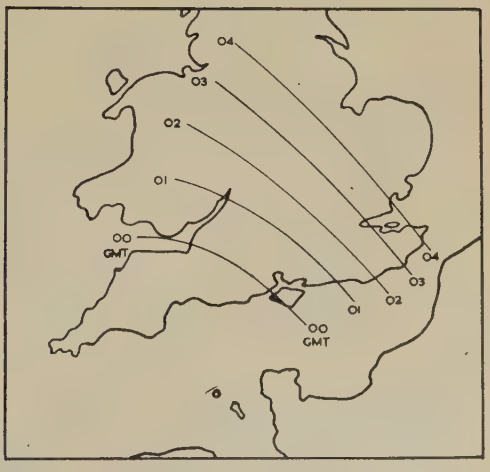


Figure 4. Hourly isochrones of the pressure disturbance, 5 July 1952.

the English Channel, between Guernsey and the English coast; setting up an oscillation in the discontinuity. This oscillation was then propagated upstream of the lower flow and across the upper flow as a wavelike disturbance of sufficient magnitude to cause the surface pressure and wind variations recorded at many stations.

After the initial outburst of cold air the easterly flow was re-established over the Channel, relieving the temporary blockage of the lower flow on the coast.

5. Pressure effects associated with the disturbance

Barograms were examined for 35 stations in England and Wales, south of the Humber. A distinct pressure oscillation was shown on 29 records, those which showed no effect being in extreme south-east England, and in Cornwall where the trace was extremely unsteady throughout the night. No effects were recorded in Wales, or north-east of a line from the Mersey to the Wash.

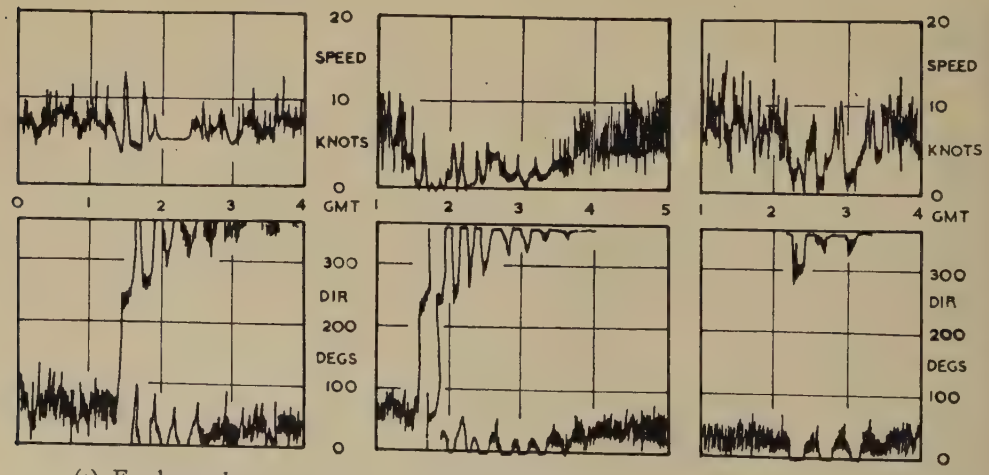
The time of the start of the pressure oscillation was determined for as many stations as possible from the barograms, and the anemograms where these were available. This timing was subject to an error of about five minutes, assuming that the records had been correctly time marked.

The times were plotted and hourly isochrones drawn which showed that the source of the disturbance was over the western English Channel, and that the disturbance moved north-eastward with decreasing speed and a decrease in the amplitude of its effects.

In each case the pressure oscillation began with a sudden rise followed by an oscillatory decay to the original pressure which lasted for about 90 minutes (Fig. 3). The traces are extended through the morning of 6 July to show the effect of the outflow of cold air at the surface as thunderstorms moved over or near the stations. No thundery activity occurred over England away from the South Coast during the night 4-5 July.

6. Wind velocity effects of the disturbance

Dines pressure-tube anemograms were examined for 11 widely-spaced stations. Nine of the records showed a marked effect associated with the oscillation of pressure. No effects could be traced at St. Merryn in Cornwall, or at Aberporth in Wales.



(a) Farnborough. (b) Abingdon. (c) Croydon.

Figure 5. Anemograms 5 July 1952.

The three individual records with the most marked effects are described below. The other records show that the surface flow was reversed for a time at Calshot, Hampshire, and at Henstridge, Dorset, although a marked periodicity appeared in the speed at Calshot. At Boscombe Down and Thorney Island the surface flow was temporarily blocked, but at places further away from the coast a periodicity in both speed and direction was imposed on the mean flow. There is some evidence of a periodicity as far away as Bramcote, Warwickshire.

(a) *Farnborough, Hampshire*

The Farnborough anemogram (Fig. 5a) was first affected at 0130 GMT when the direction veered from 070° through south-west to 070° in 18 min, while the speed rose to a sharp maximum of 13 kt. The direction then oscillated about north for four periods with amplitudes of 180, 130, 110 and 100 degrees before finally settling down to the original speed and direction at 03 GMT.

Apart from the first 13-kt gust there was only one other pronounced maximum of speed, which was a gust of 12 kt when the wind backed to a turning point at 270° in the first period. The speed remained remarkably steady at 5 kt from 0200 GMT until 0230 GMT when the original speed of about 8 kt was resumed.

(b) *Abingdon, Berkshire*

The direction periodicity and the first sudden veer through a complete circle (Fig. 5b) are similar to that recorded at Farnborough (Fig. 5a). A further turn through 360° occurred under almost calm conditions before the periodicity became established.

The speed maxima coincided with the periodic swing through the original direction of north-east, falling almost to calm for any other direction.

In the direction trace six full periods oscillating about north are clearly shown, with successive direction amplitudes of 170, 160, 110, 80, 70 and 50 degrees. The period of the oscillations slowly increased from 15 to 20 min.

(c) *Croydon, Surrey*

At 0215 GMT, from steady conditions with a speed of 8 kt from a direction of 030°, the direction backed to 290° and the speed fell off to less than 2 kt.

During the following 90 min there was a steady periodic variation in direction and speed with a periodic time of about 20 min (Fig. 5c).

The speed maxima of about 10 kt coincided with the periodic swing through the original direction.

7. Discussion

The reversal of the surface flow at Calshot, combined with a periodic oscillation of the speed, shows that the cold air flowing outward from the thunderstorm area cannot have been far to the south-west.

At Boscombe Down, 30 mi to the north-west of Calshot, the surface flow was temporarily stopped by the downstream block. A periodic oscillation of wind superimposed on a steady flow was nearly realized at Thorney Island, 20 mi east of Calshot, although a prolonged gust to a maximum speed of 28 kt, from a direction across the flow beneath the inversion, indicated that some effect other than a purely periodic oscillation was present, at least for the first 15 min.

A periodicity superimposed on the surface flow was well established at Farnborough, Abingdon and Croydon, and it is over this area, at least 50 mi from the estimated closest approach of the cold air, that the behaviour of the discontinuity is discussed.

The Larkhill radiosonde ascent at 03 GMT, 5 July, which was representative of the air over this area, showed two well-marked inversions (Fig. 2). Unfortunately no winds were available from that ascent to show which inversion coincided with the boundary between the upper and lower currents. An objective estimate of the mean velocities of the upper and lower airstreams over the area was made assuming that the upper inversion could be considered coincident with the wind discontinuity.

8. Analysis

The problem may be treated simply as an analysis of the behaviour of the discontinuity between two superimposed streams in an incompressible fluid, with the lower stream, of depth h, density ρ, and velocity U flowing over a rigid surface. The upper stream is assumed to be unlimited in depth.

The following relations between variations of wind speed and pressure at the surface are obtained (Goldie 1925) :

$$\Delta p_s = \frac{\rho a}{h}(U\cos\beta - V)^2 \quad . \quad . \quad . \quad . \quad (1)$$

$$\Delta U_s = \frac{a}{h}(U\cos\beta - V) \quad . \quad . \quad . \quad . \quad (2)$$

where Δp_s is the amplitude of the surface variation of pressure, ΔU_s the amplitude of the surface variation of wind speed, a the amplitude of the wave motion of the discontinuity, β the angle between the direction of the lower airstream and the wave motion, and V is the velocity of the wave motion.

It is emphasized that the assumption of incompressibility, and the concentration of all the stability at the inversion, while giving equations that can easily be handled and applied, is a great over-simplification of the atmosphere, but in the present case a comparison between the theoretically deduced speed of the lower airstream and an objective estimate of the actual value showed an interesting measure of agreement.

The theoretically deduced amplitude of the wave motion of the discontinuity is therefore not likely to be greatly in error.

The following values were taken as common to Farnborough, Abingdon and Croydon:

$$h = 1{,}650 \text{ m},\ U = 10 \text{ m/sec},\ \beta = 145°, \text{ and } \rho = 1{\cdot}13 \times 10^3 \text{ g/m}^3.$$

Table 1 gives for Farnborough, Abingdon and Croydon, the values observed for various parameters and then deduced from Eqs. (1) and (2).

TABLE 1.

	(a) Farnborough	(b) Abingdon	(c) Croydon
Deduced from autographic records			
Velocity of the wave motion, V (m/sec)	13	16	11
Period of oscillation of wind direction, T (sec)	1,020	750	1,200
Wave-length, λ (km)	13·3	12·0	13·2
$2\Delta p_s$ (mb)	1	1·4	1
$2\Delta U_s$ (kt)	8	10	9
$(U\cos\beta - V)$, (m/sec)	− 21	− 24	− 19
Deduced from Eqs. (1) and (2)			
$(U\cos\beta - V)$, (m/sec)	− 22	− 24	− 19
a (m)	162	177	201

9. Conclusion

The model used is a rough approximation to the actual condition of the atmosphere but the quantitative results show that it is by no means invalid in spite of its simplicity.

It indicates oscillations of amplitude of the order of 200 m, or about one-eighth of the depth of the lower layer, which could lead to the formation, and quick dispersal, of a stratocumulus type of layer cloud, provided that the air is near saturation at the level of the discontinuity.

This condition was present at Liverpool at 03 GMT, 5 July, and in view of the similarity of all the English radiosonde ascents at that time, the condition was probably widespread over a large area. At Filton the following cloud changes were reported during the early hours of 5 July : ' Between 0001 GMT and 0100 GMT low cloud increased from nil to six oktas, while a light NE wind veered to ESE. By 0200 GMT the wind had backed to north-east again, while the low cloud decreased to nil.'

The wave motion is estimated to have reached Filton by 0030 GMT so that the cloud reported was almost certainly associated with the oscillation.

The amplitude of the wave motion demonstrates its capacity as a means of transferring energy from small-scale weather systems quickly over large distances, irrespective of the wind field.

Oscillations on this scale may have some bearing on the variability of frontal rain and the case analyzed is a particular example of a situation which is very similar to a warm front. If latent instability is present in the warm air it is possible for a similar process of energy transfer to be responsible for rain breaking out, or intensifying, in an otherwise unpredictable manner.

Acknowledgment

This paper is published with the permission of the Director of the Meteorological Office.

Reference

Goldie, A. H. R. 1925 *Quart. J. R. Met. Soc.*, **51**, p. 239.

551.515.7 : 551.513 : 551.557

A study of blocking in the Atlantic-European sector of the northern hemisphere

By E. J. SUMNER
Meteorological Office, Dunstable

(Manuscript recevied 16 December 1953, in revised form 26 May 1954)

SUMMARY

Various interpretations of what is meant by ' blocking ' in the atmosphere are discussed. The three main aspects referred to by writers on the subject are : (a) a sharp local diminution of the mean zonal (tropospheric) flow, often associated with a bifurcation of the jet stream, which diminution is (b) rather persistent and (c) displaced westwards with time. Not all of these attributes have been insisted upon as defining blocking but usually (a) and (b) are required. The view is taken herein that, for forecasting purposes, only (a) is admissible, and a selection of instantaneous patterns having this characteristic is made, two broad types being distinguished, for the period 1949-52 and in the Atlantic-European sector of the northern hemisphere.

Some statistics relating to the seasonal and geographic distribution of these patterns are given, which in the main confirm previous results. Blocking patterns were found to occur on more than half the days, individual blocks lasting on an average 16·5 days. They were most frequent in May and least frequent in July, and tended to be displaced from Europe to mid-Atlantic during the first six months of the year. Within this general westward drift several successive ' waves ' of progression and retrogression were in evidence. Blocking over N. America was extremely rare.

The position and movement of individual blocks is presented in some detail, and notes on their formation, dissipation and general character are also given.

1. Introduction

The purpose of this essay is to discuss and to illustrate with examples what is known as ' blocking ' or ' blocking action ' in the circumpolar westerlies, and to provide some recent statistics of the day-to-day behaviour and of the geographical and seasonal distribution of blocking, mainly in the Atlantic-European sector of the northern hemisphere.

The term blocking was introduced into synoptic meteorology in American publications as defining one of a series of models of tropospheric flow, about which general subject a considerable amount of literature now exists. The terminology and the ideas are therefore familiar and largely accepted by workers in this field, and are gradually being absorbed into the language of synopticians. However, in connection with blocking certain discrepancies of usage still exist, which call for some discussion if not re-definition.

The idea of a surface anticyclone in middle latitudes preventing – or blocking – the eastward progress of oncoming systems is very old in synoptic meteorology, and is presumably behind the introduction of the word ' blocking ' as a technical term. In recent years the increase in aerological data has enabled this phenomenon to be examined in its proper three-dimensional context, resulting in a more precise connotation in which mid-tropospheric flow patterns are especially emphasized. Various definitions or descriptions of blocking behaviour are to be found in the literature, a representative selection of which is now given.

2. The idea of blocking

Namias (1947) describes blocking in terms of a ' retardation ' (diminution) of the zonal circulation in a limited sector of the hemisphere and at all levels in the troposphere, *which retardation spreads westwards.* This, it is said, is usually a slow process sometimes lasting for weeks and affecting large areas; several ' waves ' of blocking may take place in fairly close succession. The 'surface ' systems in the blocked sector usually comprise

a warm anticyclone in high latitudes and a cold depression or depressions in lower latitudes (an extreme low-index type). These particular features may be maintained in the same longitude even though the local diminution of zonal flow spreads westwards, but in general are themselves transferred upstream. The day-to-day synoptic developments and individual systems associated with this westward transfer (retrogression) are often complex, so that it is sometimes difficult to detect without special smoothing aids, such as mean charts.

Elliott and Smith (1949), although they do not explicitly include retrogression in their definition, illustrate the blocking process with the developments of 19-27 February 1944, when retrogression did occur. They define blocking as

> A state of circulation in which the normal zonal flow is interrupted in a sector . . . by strong persistent, meridional-type flow. Synoptically . . . a persistent high at high latitudes . . . obstructing the normal eastward progress of migratory cyclones and anticyclones. The high is linked, as a rule, to simultaneous abnormally deep depressions either upstream or downstream or both, which are often trapped in low latitudes.

Berggren, Bolin and Rossby (1949) seem to accept Namias's definition, retrogression included; they attempt some sort of explanation of the initiation and development of blocking in terms of instabilities in the normal zonal circulation, leading to the abnormal growth of upper waves and to the partial cutting off of warm anticyclonic and cold cyclonic vortices by a sort of large-scale seclusion process. They emphasize the complex and discontinuous nature of the ' westward advance ' as depending on the rapid development and gradual stagnation, each time further west, of a series of waves *from* the west. The upper flow pattern is characterized by a splitting of the main zonal current (the jet stream) into two distinct branches, one being deflected north round the anticyclonic vortex and one to the south round the cold low. In the particular case illustrated (12-20 February 1948) ' the region of branching pushed steadily westward, dividing and destroying the zonal motion behind it.' This was a slow process, taking eight days to cross the Atlantic.

Rex (1950) insists on a more rigid delineation of his blocking model, with a clear-cut upper jet which (at 500 mb at least) splits abruptly into two definite streams, in every case. Downwind of this bifurcation there is usually a ' macro-turbulent region ' a main feature of which is ' a quasi-stationary warm ridge or anticyclone.' This (upper) ridge or anticyclone may move eastwards or (more slowly) westwards, or remain essentially stationary, in company with the surface high – usually known as *the* blocking high – with which it is associated. He makes the points that within the blocked sector the zonal index may either increase or decrease with blocking, depending on the latitude band with respect to the ' axis ' of the block for which it is computed. For a wide enough band – one including both limbs of the split current – there may be little longitudinal change in the strength of the mean zonal flow. He also distinguishes between blocking in the above sense and the complete cutting off from the westerlies of a cyclonic vortex without any interruption of the zonal circulation.

Bretzowsky, Flöhn and Hess (1951) include quasi-stationary wedges lying across the westerlies, as well as anticyclones, in their selection of blocked situations; although not explicity defined, they were concerned with particular types of *Grosswetterlagen*, identified on surface charts alone.

All of the above writers refer to blocking as a large-scale process, usually embracing several synoptic systems which themselves may be amongst the largest of their kind, and all include an element of persistence in their definition. Elliott and Smith, and Bretzowsky *et al.*, stipulate three days and Rex ten days as a minimum persistence; the remaining authors were more concerned to illustrate the blocking process synoptically and do not give a precise figure.

50°N.
70°N.
30°W.
0°
30°E.
500mb. contours
L
H
16,200
17,000
16,600
18,000
17,200ft.
19,000
Isobars every 8mb.
Thickness lines every 200ft.
Surface and 1000-500mb. thickness
16,800ft.
17,400
1032
1016
1000
976
984mb.
992
1024
16,600

Figure 3. Miscellaneous examples of blocking patterns.

It should be mentioned in passing that there seems to be fairly general agreement as to the importance of blocking in the general circulation problem, viz., as a regime in which the largest meridional exchanges of heat and momentum are effected. Up to the present however only crude estimates of the day-to-day latitudinal transport of these properties have been made.

3. The present approach

To the present writer the essential characteristic of blocking – one that is implicit in all the foregoing definitions – is the local and, in well-developed cases, rather sharp diminution of zonal flow within the band occupied *elsewhere and previously* by the main concentration of westerlies. This may manifest itself synoptically in many ways perhaps the most typical of which is that associated with the ' split-jet ' cellular pattern, although the large-amplitude, upper-ridge pattern, which often disrupts into the former (or vice versa) and sometimes has symptoms of split flow at an early stage, is not uncommon. Good examples of these two types as they appear on surface, 1,000-500 mb thickness and 500-mb contour charts, are shown in Figs. 1. and 2; convenient names are ' difluent ' and ' meridional ' blocks, respectively. However, other patterns not so clear-cut or frequent, a few examples of which are given in Fig. 3 (500-mb patterns), are by the above token equally to be classed as blocking patterns. It should be recognized that these patterns can exist in every degree on the synoptic scale : in borderline cases this may involve some confusion between various types (e.g. between a large amplitude long-wave and a meridional block, or between cutting-off and blocking), but this is inevitable in any synoptic classification and need not be seriously misleading in a given context.

There is little doubt that blocks involving the larger-scale synoptic features *are* rather persistent, but miniature blocking patterns of short duration do occur. Also, although retrograde blocking is common enough, patterns which in other respects everyone would call a block often remain quasi-stationary or move eastwards, as Rex (1950) has sufficiently emphasized. It is true that in some such cases the flow upwind does slacken with blocking symptoms appearing in it, but whether this is a direct effect of the original block has never been objectively demonstrated.

For some statistical purposes it is perhaps justified, even necessary, to stipulate a minimum persistence or size and a particular type of pattern or behaviour (indeed we shall have occasion to select a sample of the total population of blocking patterns in the following study). However, in general synoptic work it is desirable to preserve a more elastic outlook. An important requirement of the practising synoptician is to be able to describe in definable terms and on every scale the patterns and evolutions which appear on his working charts; but not in such terms as imply a forecast of future changes, unless it has been previously established that certain sequences are (almost) invariably followed.

Thus our attitude here is that a block is best defined as an instantaneous state of circulation, e.g., as a blocking *pattern*, and that it is preferable to speak even of blocking or blocking action in a similar sense rather than as a process having a preconceived duration or behaviour – such aspects can readily be introduced by qualifying the word blocking. No rigid model is advocated or considered to be possible, although some characteristic shapes, such as those of Figs. 1-3 are acceptable norms.

We now proceed to a selection of blocking patterns with a view to obtaining information about their seasonal and geographical distribution, general behaviour, modes of formation and dissipation, etc. These statistics in the main confirm those of other workers, but not in all details, and will provide a basis for further discussion.

4. The procedure and basic data

The following data are for the 4-year period from January 1949 to December 1952, inclusive, and for the area from 100°W eastwards to 60°E and north of 50°N. This area is the one likely to be of most importance in extended forecasting for the British Isles.

The selection of the appropriate patterns and the measurements were made mainly on miniature circumpolar charts (03 GMT surface, 1,000-500 mb thickness and 500-mb contour); however, these were not available for the first three months of 1949 and for this period the *Synoptic Weather Maps* (sea-level and 500 mb) of the U.S. Weather Bureau were used.

In the main, patterns reasonably resembling those of Figs. 1 and 2 were chosen, allowing for the almost inevitable smaller-scale irregularities. Both the surface and the upper (500 mb) cellular structure had to be fairly well formed, although a particularly clear-cut upper pattern was sometimes deemed to compensate for a poor surface pattern and conversely. Also a short temporary waning, lack of definition or distortion in the patterns was sometimes ignored for the sake of continuity (Figs. 3a and 3c are, for example, both taken from the present selection). Blocking was said to have begun as soon as such a clear-cut pattern appeared on an 03 GMT chart, and to have ceased with the last 03 GMT chart of a spell showing this structure. Both the selecting of a block and the precise demarking of a spell of blocking, are obviously rather subjective processes. However, the charts were examined several times at fairly lengthy intervals apart, with an eye to uniformity of selection; and the agreement with comparable statistics of other investigators encourages one to believe that an acceptable measure of objectivity has been achieved.

It is considered that the most reliable and definitive feature of blocking patterns is the upper ridge or high, and in positioning blocks the following convention was therefore adopted. If the upper anticyclonic vortex (or if absent the surface high) was well-formed and fairly symmetrically placed with respect to the upper ridge, then its latitude and longitude were taken. If the surface or upper cells were not very well associated with the ridge (sometimes the surface high is displaced into the baroclinic zone around the upper ridge) then an attempt was made to fix a position in the weakest westerlies in the ridge, bearing in mind the day-to-day continuity. All these measurements were made to the nearest 5°.

No restriction was placed on the (minimum) duration of the patterns : occurrences on a single 03 GMT chart would have been selected, although in the event no such cases were found.

If a block moved *longitudinally* into or out of the specified area at some time during its life-time, it was ' followed ' in order to complete the statistics relating to the duration of spells. In most other statistics, such as those dealing with the annual and geographical distribution of ' days of blocking ' (occurrences on 03 GMT charts), those periods which the block spent outside the area in question were excluded.

The area chosen was rather arbitrarily delimited to the south by the 50°N parallel, not because blocking patterns located south of this latitude do not exist, but, because of observational shortage in low latitudes, one cannot always identify them. Also, there is some evidence that the low-latitude jet in the Atlantic-European sector often has its main thermal concentration above the 500-mb level, and a block of the type shown in Fig. 1, although quite definite at 300 mb, say, may be difficult to pick out on 500-mb charts even if there were plenty of observations. Another point is that blocking in such low latitudes is effectively little different from the more normal westerly type in the latitudes in which we are mainly interested here.

Blocks which formed well south of 50°N, but which quickly surged northwards from the sub-tropical belt were included in the selection. The rare occasions of low-latitude

blocking varying in position around the 50°N-boundary were excluded. A spell of blocking was discontinued if and when it finally sank below 50°N. The shape of the frequency distribution of blocking with latitude (Fig. 7, later) supports the idea that very little has been lost by this latitudinal differentiation.

An attempt was made to classify the longitudinal movement of blocking patterns into the three classes : eastward moving or progressive (P), quasi-stationary (Q) and westward-moving or retrogressive (R) patterns, respectively. In view of the complex and discontinuous nature of some of the displacements (in cases of retrogression, especially) and the crudity of some of the measurements, the following rough criteria were adopted. Movements sustained for at least three days, of any magnitude but consistently east or west, were classified in the P or R classes, respectively (by far the greater proportion of these averaged 5° long./day, or more). Movements lasting two days or less had to be more than 5° long. in all, to be placed in one of these classes. The remaining periods were all placed in the Q class. The measurements of position were recorded only at the beginning and end of such periods, unless marked changes of zonal speed or latitude occurred within them.

The purpose of the above selection and measurements was not to give a pretence of exactitude, but to permit of some general statements of a statistical nature based on something more than mere impression or experience. A lengthy technique could be developed which would make the whole procedure more objective and more precise, but it is doubtful that this would give a worthwhile return.

5. Blocking 1949-52

(a) *General statistics*

The occurrence of clear-cut blocking within the 4 years in questions is shown in Fig. 4; sub-periods of progression, stagnation* and retrogression are distinguished. The longitudinal positions of the blocks are given for the beginning and end of a spell, and at some intermediate points for the longer spells.

On some 5 per cent of occasions two blocks occurred simultaneously within the area; in the following statistics these were treated as two separated days of blocking, in some cases the number of days involved being given. Also the final case, commencing on 26 December 1952, continued to 6 January 1953, and the last six days, during which the block was progressive, have been included in some of the totals.

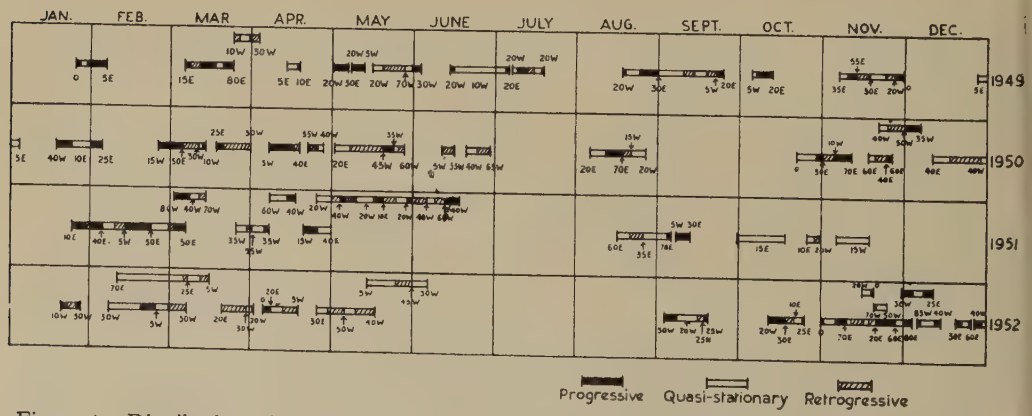


Figure 4. Distribution of blocking : Jan. 1949—Dec. 1952. Area : 100°W-60°E; north of 50°N.

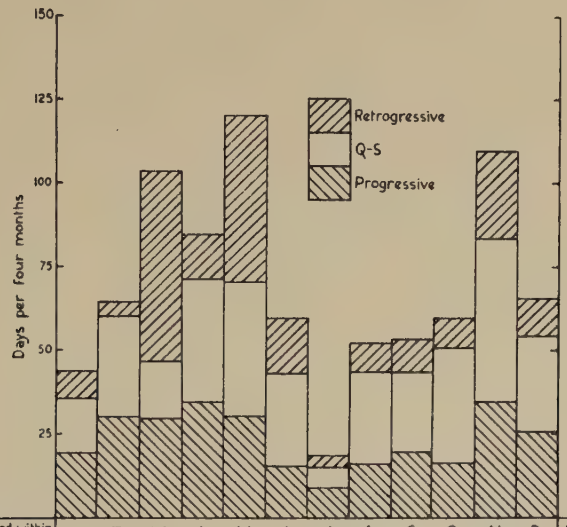
* The word ' stagnation ' is used, although not particularly appropriate, for want of a better term.

The total number of days of blocking was 878 : 296 with progressive patterns, 364 quasi-stationary and 218 retrogressive; however, only 835 of these were for blocks actually situated within the area considered, of which 45 days were over-lapping. They were arranged in 53 distinct spells of average duration 16·5 days.

(b) *Calendar and geographical distribution*

The monthly distribution (including overlaps but not blocks outside the period or area) is shown in Fig. 5, together with the relative occurrence of days of progression, stagnation and retrogression. The general profile, with its May maximum and July minimum, is in close agreement with that given by Rex (1950): 14 years' data for a similar sector, from the end of 1932 to 1940 and from 1945 to the beginning of 1950, and Bretzowsky *et al.* (1951) : 70 years' data, 1881-1950, for the sector 20°W-50°E. The secondary maximum in autumn is not supported by Rex and only shows up weakly in Bretzowsky's data.

There was little significant change from month to month in the proportion of days of progression : retrogression was relatively most frequent in March, with May a close second and June and November next in order. A discrepancy is noted here with Rex's data which showed no retrogression at all in May and June.* Even a rough comparison is difficult, however, since he was concerned with the *net* displacement from the beginning to the end of a spell, the blocking patterns being located with respect to the ' split ' in the upper flow. However, on his criterion a large amount of retrogression would still have been found in May in our data : there can be little doubt, for example, that the good



Spells centred within the month in question	J	F	M	A	M	J	J	A	S	O	N	D	ALL
Number	3	3	7	6	7	3	1	2	3	5	8	5	53
Average duration: days	14·0	36·0	15·0	10·0	22·5	12·5	13·0	22·0	21·0	14·0	15·0	11·5	16·5
Average speed within periods of progression Deg. long./day	6·5 (7)	5·0 (5)	7·0 (8)	5·5 (8)	5·0 (7)	3·0 (3)	6·0 (3)	5·0 (3)	7·0 (5)	6·5 (4)	7·5 (10)	6·5 (8)	6·0
Average speed within periods of retrogression Deg. long./day	12·0 (3)	11·5 (1)	7·5 (12)	9·0 (3)	6·5 (8)	5·5 (3)	10·0 (1)	8·0 (2)	9·0 (3)	6·5 (2)	6·5 (7)	7·5 (1)	7·0

Number of periods in brackets

Figure 5. Monthly distribution of blocking.

* There seems some doubt about the June figures; Rex's ' blocking case catalogue ' includes retrogression for 14-25 June 1939 from 5°E to 40°W, which is not included in the table of block movements.

example of retrograde blocking of May 1950 (just outside his period), would have been so classified by Rex. The case of May 1949, included here, was rejected by him, presumably because it was in very high latitudes and at times had very little splitting of the upper current, most of the flow crossing the Atlantic in middle latitudes south of the warm anticyclone to the north (see Fig. 3c). Thus there appears to be a real difference between the two periods, but possibly accentuated by the different criteria of selection and measurement.

The longitudinal movement of individual blocks, somewhat smoothed, is plotted in Fig. 6. The most obvious features are the general displacement of blocking westwards from Europe to the Atlantic from about February to June, and the westward 'spread' from August to December. These broad seasonal trends are consistent with the findings of Bretzowsky *et al.* (1951), who were the first to point out that the annual distribution of blocking was very dissimilar over the Atlantic and over Europe (their data were segregated west and east of 10°E, respectively). One small point of difference is that their Atlantic maximum occurred in early June (see Fig. 2, Bretzowsky *et al.* (1951)), not in May as in the present data.

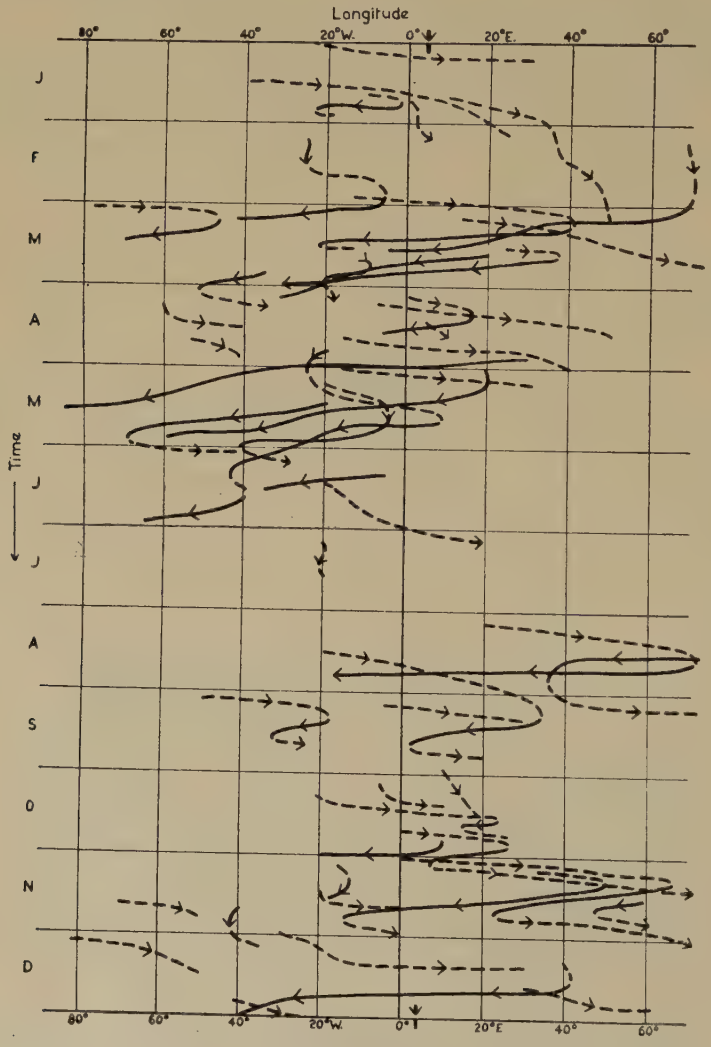


Figure 6. Longitudinal movement of blocks.

Within the first half of the year, successive waves of progression and retrogression are in evidence; these may, however, only be a feature of the present period. Rex (1950) gives the initial, final and mid-point longitudes of his blocks, and these were plotted in a similar way to Fig. 6. Several drifts and counterdrifts were present (not shown here), but of less amplitude and not in line with the present data. Moreover, the general westward displacement of blocking during spring and early summer was not very much in evidence. However, as previously stated, a true comparison is difficult : the position of the jet exit is in general not considered to be such a sensitive indicator of movement as that of the blocking ridge, which in extreme cases may move in opposition to the difluence.

(c) *Distribution with latitude and longitude*

The statistics of the monthly distribution of days with a block situated within narrow sectors are tabulated in Appendix 1. The monthly totals east and west of the Greenwich Meridian, representing a rough division into European and Atlantic data, are also shown. Except for the occurrence of the Atlantic maximum in May rather than early June, as remarked above, the figures in the appropriate sectors agree with those of Bretzowsky.

In Appendix 2 the corresponding statistics with respect to latitude are given. Variations from month to month are irregular and often small; however, some general ' lifting ' to higher latitudes from winter to summer is apparent, with superposed peaks for the months when blocking is also most frequent and furthest west. Within the individual spells short-period variations – gradual trends and irregular fluctuations – in latitudinal position were continually occurring; however, by far the greater number of these were only of the order 5-10°. In fact 32 of the 53 spells varied in latitude throughout their life only by 10° or less (these included most of the shorter spells, but also three spells of 22 days and one each of 26 and 43 days; see Fig. 9); 14 spells fluctuated by as much as 15° of latitude and the remaining 7 by about 20°. There was a tendency for blocking patterns to move to higher latitudes within a few days of initiation and to lower latitudes as the spell was ending : 18 spells showed an initial rise of 10°-15° of latitude, while 20 ended with a similar fall.

The general latitudinal and longitudinal distributions are shown in Figs. 7 and 8 (days of blocking within the area), together with the relative proportions of days of progression, stagnation and retrogression. For all the data, progression is proportionately rather more frequent in low latitudes than high, and vice versa for retrogression; the quasi-stationary class has about the same relative frequency at all latitudes. The latitudinal data were originally segregated east and west of Greenwich, but little significant difference in the profiles was apparent or in the relative proportions of P, Q and R with latitude.

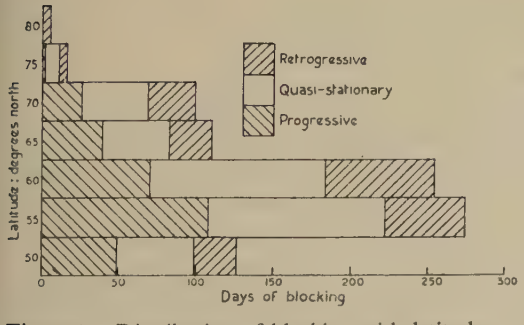


Figure 7. Distribution of blocking with latitude.

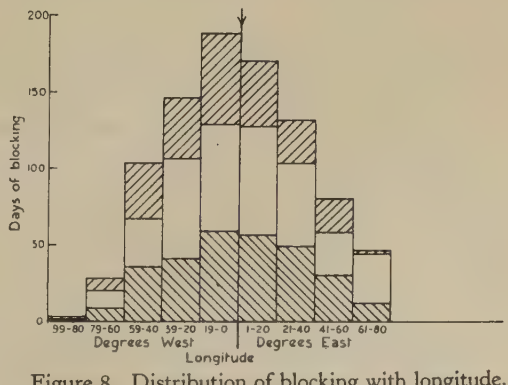


Figure 8. Distribution of blocking with longitude.

There is a peak of blocking (Fig. 8) just west of Greenwich, falling more sharply to the west than the east. Retrogression is the more frequent over the Atlantic and progression over Europe, but only slightly so. The almost complete absence of clear-cut blocking over the U.S.A. is noteworthy : large surface anticyclones with depressions trapped in lower latitudes do occur in this sector, but the upper flow remains relatively undistorted and fails to show the characteristic blocking pattern in any degree.

(d) *Duration and constitution of spells of blocking*

The distribution of spell-durations is shown in Fig. 9. They range from 5 to 54 days' duration, with a mode near the lower extremity; however there is no really decisive decrease in frequency until after 22 days. It has been suggested by Yeh (1949) that the persistence of blocking is likely to increase with latitude; accordingly a dot-diagram of length of spell against the mean latitude was plotted. There was practically no relationship; the most that could be said in favour was that all but one of the 11 spells of 7 days' duration or less were south of 60°N whereas the longer spells frequently occurred in higher latitudes.

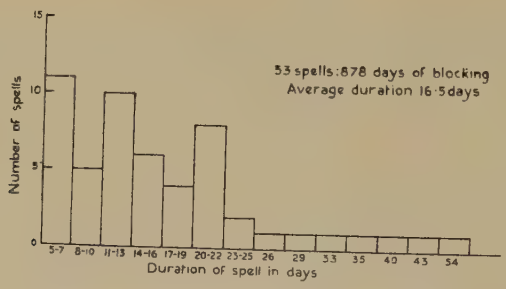


Figure 9. Frequency distribution of spell duration.

It will be evident from Fig. 4 that periods of progression, stagnation and retrogression are rather haphazardly distributed throughout the spells. In fact, a period of stagnation is almost equally likely to be succeeded by movement to the east or west, and retrogression by stagnation or progression. Progression, is, however, not quite so prone to sudden reversals. 25 spells had all three ingredients, P, Q, and R, and 21 two ingredients, some of them more than once; the remaining seven were 'pure' spells. The frequency distribution of these individual sub-periods is given in Table 1.

Periods of progression and stagnation are almost equally frequent, and are much more frequent than those of retrogression. However, on an average, stagnation is more

TABLE 1. FREQUENCY DISTRIBUTION OF PERIODS OF PROGRESSION (P), STAGNATION (Q) AND RETROGRESSION (R), OF VARIOUS DURATIONS

Type of movement	Duration of period (days) 1	2	3	4	5	6	7	8	9	10	More than 10 days : length given	Total No. of periods	Total days	Average (days)
P	4	13	17	10	5	12	6	2	1	1		71	296	4·2
Q	1	10	11	15	12	7	2	1	2	–	11, 12, 13, 14, 17, 18, 23	68	364	5·4
R	–	7	8	13	4	7	2	1	–	1	11, 12, 12	46	218	4·8
Totals	5	30	36	38	21	26	10	4	3	2	10	185	878	4·8

lasting than progression (5·4 days as compared with 4·2 days), and on the whole, blocking patterns are more frequently quasi-stationary than progressive. Retrogression, although its average duration (4·8 days) is greater than for progression, is still least frequent on the whole (see totals).

On investigation it was found that the longer spells (20 days or more) had a similar distribution of P, Q, R to that of the above Table, but with a slight (and scarcely significant) increase in average duration in each class. However, there was a greater preponderance of R types, the actual number of ingredients being 26 P's, 35 Q's and 29 R's. There is a slight suggestion here, and from the above Table, that retrogression and stagnation are more consistent with longevity than is progression.

The number of sub-periods commencing in each month, and the corresponding monthly mean speeds of progression and retrogression are given at the foot of Fig. 5. These mean speeds show little significant variation throughout the year from their annual means of 6·0° and 7·0° long./day, respectively. Individual values in both clasess were fairly closely grouped within 2-3° of their appropriate means, the (rare) extremes being 2° and 20° long./day. The more marked, and slightly smoothed, trends present in spring (Fig. 6) each have a mean speed of about 5° long./day, either eastwards or westwards as the case might be.

(e) *Formation, dissipation and general character*

An attempt was made to classify from the synoptic charts the ways in which these blocking patterns were formed and dissipated. To do full justice to the subject would in itself require a lengthy paper with many illustrations, and only a few notes and rough statistics are offered here.

During the formative stage there was usually a period of large-scale change or development, involving one or more of several processes, followed by a period of consolidation. The main processes in the earlier stages were : (a) an instability development, in which a small mobile (eastward-moving) upper ridge associated with a developing surface ridge or anticyclone, grew progressively in size, usually slowing down as it did so; (b) a simple disruption of a pre-existing upper ridge as the high-latitude part ' toppled ' eastwards over the trough immediately downwind, with the partial cutting off of high and low vortices in the ridge and trough, respectively. These evolutions have been illustrated by Sutcliffe (1953, Figs. 2 and 6, respectively) and are sufficiently well known not to need further illustration here.

Of the 53 spells, 10 were largely the outcome of a pronounced instability process and 5 of a simple disruption; in a further 10 a combination of both processes was very much in evidence. The lifting of a low-latitude block to higher latitudes occurred in 11 cases, all of them associated with an instability development to the north, which in six cases was quite pronounced. The remaining 17 cases were not so easily classifiable; although most of them showed minor instability or disruptive characteristics, the typical blocking pattern could best be said to emerge or consolidate *in situ*.

Pure instability developments usually led to the formation of meridional blocks (similar to Fig. 2) and long-wave disruptions to the formation of difluent blocks (similar to Fig. 1). These processes often occurred virtually as one continuous evolution, in the above order, without first forming or settling down as a meridional-type block. Sutcliffe (1953) distinguishes between ' anticyclonic disruption ' (process b) and ' cyclonic disruption ' in which the southern part of a trough under-cuts the ridge immediately ahead of it, again with the formation of upper vortices. This latter type of disruption was never associated with block formation, but sometimes occurred when an upper trough approached an existing block from the west.

The *surface* developments leading to the formation of the blocking anticyclone were often very complex, especially in cases of upper instability. In most instances a sub-tropical anticyclone was previously present and extended into higher latitudes, either in part or *in toto*. This northward displacement seemed to be facilitated by the pre-existence of an anticyclone in polar regions from which a high-pressure cell broke away to contribute to the main growth further south. However, entire anticyclones already in the westerly baroclinic zone and moving from the west were sometimes contributory, or even largely instrumental, in establishing the blocking high, especially in type (b) developments described above. The presence of a surface depression in the sub-tropical belt often seemed to deflect an encroaching high-pressure system to the north, as Sutcliffe (1953) has emphasized in his study of new anticyclone formations. Many of his examples, for the 1950-51 period, led to the blocks considered here.

The mode of dissipation was generally more straightforward: 33 spells ended as the blocking anticyclone sank into lower latitudes, where it usually persisted, with a simultaneous decrease in amplitude of the upper ridge and a resumption of more zonal flow. Except for four cases each of retrogression and stagnation, these blocks were progressive at this stage, the upper ridge moving eastwards and allowing the blocking low, if still present, and if not itself filling, to open out into a trough. In one further case the pattern simply weakened and faded away, while in the remaining 19 the breakdown was rather more complex, the main pattern of change being a narrowing or seclusion of the upper ridge, the upper part withdrawing north-westwards in company with a weak surface high and the lower part progressing and sinking into lower latitudes, allowing the westerlies to advance in middle latitudes. In 8 of these cases the block itself was retrogressive at the time and in 8 progressive.

In a few cases a weak bifurcation of the upper flow persisted for some days after the termination of the block. Also, although a general trend of weakening of the pattern was sometimes apparent days before the end, there were cases in which the maximum shapeliness and intensity occurred almost immediately before the final collapse.

Initially, 32 of the spells were classified as of the difluent type; 19 of them remained so throughout, six turned to the meridional type later and seven subsequently alternated between the two. The other 21 spells were of the meridional type at first; 8 remained so throughout, 6 turned to the difluent type and 7 alternated. Evidently, a definition of blocking which excludes either of these alternative types (which were sometimes repeated several times within a spell) is undesirable.

Anticyclonic disruption (which was by far the commoner) was much more a concomitant of progressive blocking than cyclonic disruption was of retrogressive blocking. Retrogression if it was accompanied at all by the cutting off of the trough immediately upwind of the block (a typical feature of cyclonic disruption), was more frequently accomplished by a further ridge from the west disrupting anticyclonically across the upwind trough and simultaneously merging with and effectively displacing the blocking ridge to the west. This process of discontinuous retrogression has been well-illustrated by Berggren *et al.* (1949), and Sutcliffe (1953, Fig. 6).

The location and behaviour of the blocking patterns at the times of their first and last appearances, is given in Table 2. Blocks, especially those which were initially progressive, formed much more readily on the Atlantic than elsewhere (instability formations almost invariably occur there); those which were finally progressive were, at the end-stage, more usually situated east of Greenwich, while retrograde blocking almost invariably expired to the west of this meridian. Blocks were as frequently progressive to start with as quasi-stationary, but were rarely retrogressive. They seldom dissipated unless they were moving, more usually progressively.

TABLE 2. THE LOCATION AND BEHAVIOUR OF BLOCKING PATTERNS, INITIALLY AND FINALLY

Type of movement	Longitude (degrees west) 99–80	79–60	59–40	39–20	19–0	Longitude (degrees east) 1–20	21–40	41–60	61–80	Total	Net displacement	
At beginning of spell												
P	2	1	4	5	9	2	2			25	26	First
Q		1	2	7	4	6	3	2	1	26	16	half of
R					1		1			2	11	spell
Total	2	2	6	12	14	8	6	2	1	53		
At end of spell												
P			3	8	4	4	7	3	4	33	22	Second
Q			3		2	2	1			8	10	half of
R	1	3	1	4	2		1			12	21	spell
Total	1	3	7	12	8	6	9	3	4	53		

The nature of the net displacements in the first and second halves of the blocking period is also given in Table 2 (Q in this connection represents a net displacement of 5° long. or less, P and R a greater displacement, east and west, respectively). This is to facilitate comparison with Rex (1950) who, adopting a similar convention, says : ' The motion of blocks is, in general, irregular although a trend towards retrograde motion during the developing stages and towards progressive motion during dissipation may be detected.' Neither of these trends appears in the present data, in fact a net progression is more frequent during the developing stages (i.e., within the first half of a spell). During dissipation (the second half of the period) retrogression and progression are almost equally frequent, although, as we have seen, at the very last progression is predominant.

6. FINAL REMARKS

The importance of blocking in forecasting, as a conservative broad-scale background for short-period forecasting and as a sizeable interlude in the seasonal change for long-period forecasting, is manifest from these data. The British Isles are especially liable to be affected by blocking, particularly in May as witness (Lamb 1950) the almost complete absence of long westerly spells in that month during the last 50 years, and as shown by other statistics referred to in previous sections. The effect on the weather of this country varies, depending on the precise location and behaviour of blocking systems in different years, not to mention variable iregularities on the smaller scale.

The general annual distribution and behaviour as illustrated in Figs. 5 and 6, is particularly interesting and needs explaining. It obviously depends on the thermal, frictional and dynamical effect on the atmosphere of a combination of seasonal and geographical factors with a twice-yearly optimum. But without a more exact appreciation than is at present available of the nature and amount of these effects, no explanation can be very convincing. There is little doubt that this broad distribution of blocking is an almost invariable, even essential, component in the general seasonal change of the present climatic era. The May maximum and July minimum show up in the Pacific too (Rex 1950), and must rank among the most outstanding singularities of the northern hemisphere flow. The more detailed trends (shown in Fig. 6) are probably not all characteristic of longer periods, although this remains to be seen.

ACKNOWLEDGMENT

I am indebted to the Director of the Meteorological Office for permission to publish this paper.

References

Berggren, R., Bolin, B. and Rossby, C.-G.	1949	*Tellus*, **1**, p. 14.
Bretzowsky, H., Flöhn, H. and Hess, P.	1951	*Ibid.*, **3**, p. 191.
Elliott, R. D. and Smith, T. B.	1949	*J. Met.*, **6**, p. 67.
Lamb, H. H.	1950	*Quart. J. R. Met. Soc.*, **76**, p. 292.
Namias, J.	1947	'Extended forecasting by mean circulation methods,' U.S. Weather Bureau.
Rex, D. F.	1950	*Tellus*, **2**, pp. 196 and 275.
Sutcliffe, R. C.	1953	*Met. Mag.*, **82**, p. 163.
Yeh, T.	1949	*J. Met.*, **6**, p. 1.

Appendix 1

TABLE 3. Monthly distribution of days of blocking with respect to longitude, for the four-year period Jan. 1949-Dec. 1952

Figures in the sector 61°-80°E refer only to those blocks situated therein, which at some time during their lifetime had been located within the main sector (100°W-60°E).

Month	Longitude (degrees west)					Longitude (degrees east)				Totals east and west of Greenwich		Total within main sector
	99–80	79–60	59–40	39–20	19–0	1–20	21–40	41–60	61–80	0–99°W	1–60°E	(days)
Jan.			1	6	5	24	7			12	31	43
Feb.				16	14	11	15	8	20	30	34	64
Mar.		5	12	18	19	20	18	11	13	54	49	103
Apr.		6	17	12	5	24	17	3		40	44	84
May	2	9	18	38	36	16	1			103	17	120
June		2	23	11	22					58		58
July				8	7	3				15	3	18
Aug.					16	5	18	13	3	16	36	52
Sept.			2	12	7	10	21	1		21	32	53
Oct.					18	37	4			18	41	59
Nov.		1	15	8	20	11	24	30	4	44	65	109
Dec.	1	6	12	12	10	6	5	14	3	41	25	66
Total	3	29	100	141	179	167	130	80	43	452	377	829

Appendix 2

TABLE 4. Monthly distribution of days of blocking with respect to latitude, for the four-year period Jan. 1949-Dec. 1952, and within the area 100°W-60°E

Figures in brackets give the (additional) number of days that certain of the blocks spent outside this area.

Month : Latitude (°N)	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.	Total
50	11	21	11 (1)	21	17	4	1	10	4	2	13	9	124 (1)
55	3	29	33 (1)	41	25	10	5	13	25	18	28	38	268 (1)
60	22	7 (14)	32 (4)	17	42	15	9	10	21	22	31	1 (3)	229 (21)
65	5	3 (5)	14 (4)	5	15	8	3	9	3	12	15	9	101 (9)
70	2	4 (1)	8 (3)		21	16		8 (3)		5	20 (1)	7	91 (8)
75			1			5		1			2 (3)	2	11 (3)
80			4					1					5
Total	43	64 (20)	103 (13)	84	120	58	18	52 (3)	53	59	109 (4)	66 (3)	829 (43)
Average latitude of cases inside area	58°	56°	60°	55°	60°	63°	59°	60°	57°	60°	60°	58°	

551.558.21 : 532.59

Theory of airflow over mountains : III – Airstream characteristics

By R. S. SCORER
Imperial College, London

(Manuscript received 6 January 1954, in revised form 5 May 1954)

Summary

The problem of what second boundary condition (the first being the ground profile) should be applied to the differential equation for the variation with height of the wave amplitude is considered and it is thought that solutions corresponding to upward and downward progressing waves are generally irrelevant. In practice we shall be concerned mainly with those waves of length such that their amplitude decreases with increasing height. Only when there is a considerable range of such wavelengths is the motion of the kind supposed in the perturbation theory.

Though it is desirable to characterize an airstream by a single number this is not at present possible for airstreams in general, and it is shown by examples how airstreams may possess the same lee wave length though their l-profiles differ considerably. These examples show that generally the maximum amplitude of the waves is found at or close to levels of maximum values of l, at sharp inversions in particular.

The behaviour of lee (stationary) waves is fairly representative of mountain waves in general.

1. Introduction

In two previous papers (1949 and 1953) herein to be referred to as I and II, it was shown how lee waves were possible in certain kinds of non-uniform airstream and the idea was developed. One problem, the nature of the flow at great height, was put aside temporarily, and is now to be discussed. This discussion indicates that the chief value of the perturbation theory is in the study of lee waves – those standing waves whose amplitude tends to zero with increasing height in the upper layers, and which occur in the lee of any disturbance if the airstream can contain them.

A general discussion of lee waves is impossible on account of the great number of variables involved, and so ideas must be gleaned from particular cases. Some method of comparing different airstreams is desirable. Uniform streams of water of finite depth can be compared by their Froude number, but for an airstream in which the velocity and stability are variable functions of height – as they must be if lee waves can occur at all – no single number characterizing its properties has been proposed. Any single number must necessarily be derived as a kind of integral, throughout the whole depth of the airstream, of some sort of Froude number for each of an infinite number of infinitely thin superposed layers. Benton (1954) has discussed this question and though some progress has been made practical application to the atmosphere is still far off.

In this event some properties of airstreams are illustrated by particular cases, in the hope that they may be a guide to forecasters presented with similar airstreams.

The notation is the same as that given in II.

2. The problem of the second boundary condition

In the solution of the problem of the flow of an airstream over a mountain ridge by the Fourier integral method, or some analogous procedure, it is necessary to apply two boundary conditions to the differential equation

$$w'' + (l^2 - k^2)\, w = 0 \quad . \quad . \quad . \quad . \quad (1)$$

which governs the variation with height of the vertical velocity w, for waves of length $2\pi/k$, in the airstream in which l^2 is given at every height. The first condition is given

by the shape of the ground, which determines w at $z = 0$, say. In searching for a second condition various devices have been proposed in order to determine a unique solution to the problem.

If it be assumed that in the uppermost layers of the airstream l^2 is independent of z, and there seems to be no reason why this kind of airstream should behave in an exceptional way, then in those layers

$$w = A \exp\{k^2 - l^2)^{\frac{1}{2}} z\} + B \exp\{-(k^2 - l^2)^{\frac{1}{2}} z\} \quad . \quad . \quad (2)$$

and we have to determine A and B. When $k > l$ we put $A = 0$, because w cannot increase exponentially upwards, and there is no problem because B is then determined by the ground contours.

When $k < l$ the solution proposed in I, and used again in II, is that $A = 0$ because then the wave fronts progress upwards through the air.

All other authors disagree with this conclusion and so the various considerations will now be discussed.

(i) *Convergence of a series*

Zierep (1952), solving the problem for an airstream whose velocity profile is approximately that produced by a constant eddy viscosity, rejected the solution $A = 0$ on the grounds that in the method he used it corresponded to a divergent series. The series is, however, an asymptotic series and it was the first term of this series that was used in I. This asymptotic series is obtained by successive integration by parts of an integral that is convergent. This is a standard method of obtaining the series, but Zierep obtained it in a different way and rejected it as a physically impossible solution on account of its divergence.

(ii) *The effect of friction*

Rayleigh's classical discussion of waves on a stream of water suggests that friction might provide a criterion. A solution, in which a train of lee waves lies on the downstream side of the obstacle only, was then obtained by assuming a small amount of friction. It is equivalent to saying that the stream comes upon the obstacle with no disturbance in it, or that there is no disturbance at a great distance upstream. In this case there is one special wavelength such that its amplitude is always zero on the bed of the stream and so it is excited out of all proportion to the other component wavelengths by an unlevel bed. The solution thrown up by the mathematics, without friction, consists of a train of stationary waves to infinity in the upstream direction from the obstacle which is out of phase with a similar train on the downstream side; but, since it is permissible to add an arbitrary amount of any solution which does not alter the boundary conditions, a train of waves is added which exactly cancels the train extending to infinity upstream and doubles the amplitude of those on the downstream side of the obstacle. The introduction of a small amount of friction produces this same result.

By analogy Lyra (1943) and Queney (1947) argued that the result of friction must be to make the disturbance lie mainly on the downstream side, and when the complete Fourier integral solution is obtained this condition is satisfied by putting $B = 0$.

But there is one important difference, namely that in the solution so obtained the amplitude of the disturbance decreases both upstream and downstream so that friction (or radiation, or any other damping agent) could only be effective in determining the solution if it could produce its effect in the region where the amplitude of the disturbance is not negligible. It was subsequently pointed out by Queney (1952) that friction can only reduce the amplitude of the waves appreciably over a period of days, whereas the

air passes through the region in which the disturbance is appreciable in, at most, a few hours.

If we wished to argue by analogy and say that at a great distance upstream the solution with zero amplitude at the ground is absent then we would have to put $A = B$, for $A = -B$ corresponds to $w = 0$ at $z = 0$.

(iii) *A time-dependent solution*

Hoiland (1952) showed that if a stream of water initially at rest everywhere is suddenly made to move with uniform velocity (or, alternatively, an obstacle on the bed of the stream is suddenly started into uniform motion) then the wave train on the lee side, as found by Rayleigh, is soon established. Applying the same method to an airstream, Wurtele (1953) and Palm (1952) have deduced that if started up from rest in this way (or for that matter gradually but not too slowly) the motion would tend to that given by Queney and Lyra, namely the steady-state solution corresponding to $B = 0$.

There is no objection to this as a method of obtaining a solution of a problem, but whether this is how wave motion in an actual airstream is initiated may be questioned. The motion over a hill is of the kind supposed in the perturbation theory only part of the time. There may be standing or moving eddies on the lee side of the hill or important vortical motions up at high levels (usually described in one actual kind of motion as clear-air turbulence), and then, by some change in the nature of the airstream, wave-type motion sets in. There is no reason at present to suppose that it sets in first at the ground; indeed the Sierra Wave in California sometimes seems to behave like a hydraulic jump at low levels and to execute wave motion higher up. There is no proper analogy between the motion gradually spreading downstream to infinity and spreading upwards to infinity from an obstacle in the atmosphere because the whole depth of the atmosphere is linked by gravity and this linkage is instantaneous if sound waves are assumed to travel with infinite speed – as they are in these treatments.

The energy of the wave motion is derived from the energy of the original stream, and unless there is obviously a transport of energy from one level to another there is no reason why the wave motion should appear first at any one level. Hoiland's approach does not give a reason for rejecting one of the solutions in the steady flow case. There may be time-dependent solutions which can tend to a different steady state as time proceeds.

(iv) *The flow of energy*

Arguing that the disturbance owes its origin to the ground contour the solution in which $A = 0$ was chosen in I because waves of the form $\cos(kx - \nu z)$ are as shown in Fig. 1. The air is flowing from left to right and the wave crests are tilted in a downwind direction. This means that the planes containing the wave crests have a component of velocity upstream equal, and opposite, to the velocity of the stream, U, and a component upwards equal to $\nu U/k$. The waves therefore *appear* to travel away from the ground. If $B = 0$ they appear to travel towards the ground.

Over half a wavelength, $X_1 X_2$, the air is flowing upwards and the velocity is greater than in the half, $X_2 X_3$; where it is flowing downwards. More kinetic energy is thus flowing upwards than downwards. In this sense energy is flowing upwards, and a statement to this effect was made in I. This is not strictly correct because the velocity along $X_2 X_3$ is less than along $X_1 X_2$ and so the pressure is higher there. Thus the air below $X_1 X_3$ is having work done on it by the air above.

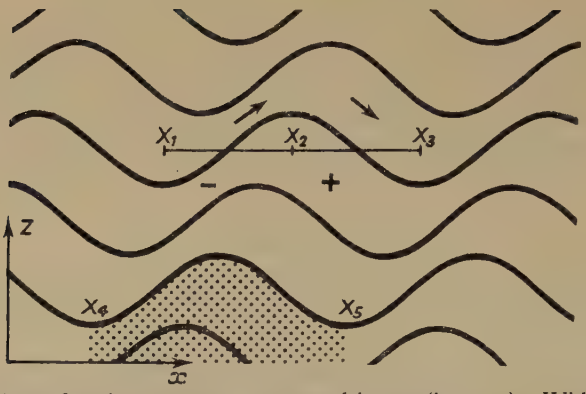


Figure 1. The streamlines of stationary waves represented by $\cos(kx \pm \nu z)$. With the positive sign the flow is from right to left, with the negative sign it is from left to right. The wave crests lie in surfaces which have a horizontal component of velocity upstream, relative to the air, and a vertical component downwards and upwards in the respective cases.

In the case of flow from left to right kinetic energy is flowing upwards but work is being done by the upper layers on the lower layers and the net flow of energy is zero.

The pressure on right half of the surface containing the streamline $X_4 X_5$ is greater than on the left half so that there is a horizontal thrust to the left on it.

When the waves are represented by $\cos kz\, e^{-\mu z}$ the crests lie in vertical planes and there is no flow of kinetic energy, no work done by lower layers on upper layers, and no thrust on a stream surface.

The mean upward flow of kinetic energy across a horizontal surface of unit area is equal to

$$\frac{k}{2\pi}\int_0^{\frac{2\pi}{k}} w\, \tfrac{1}{2}\rho_0 (U^2 + 2Uu)\, dx + \text{higher-order terms}$$

$$= \frac{k}{2\pi}\int_0^{\frac{2\pi}{k}} \rho_0 U u\, w\, dx = E \qquad . \quad . \quad . \quad . \quad . \quad (3)$$

The perturbation of pressure is $p = -\rho_0 Uu$, so that the mean rate of working on the air above a horizontal surface by the air below is equal to

$$\frac{k}{2\pi}\int_0^{\frac{2\pi}{k}} p\, w\, dx = -E \qquad . \quad . \quad . \quad . \quad . \quad (4)$$

There is therefore no total flow of energy either upwards or downwards in any of these wave motions. We must expect this conclusion because if in any layer of the atmosphere the value of l is less than k the two possible types of wave have their crests in vertical planes. In particular if there is an adiabatic lapse rate and a linear velocity profile in the bottom layer then $l = 0$ and no energy could flow upwards from the ground for any wavelength because the waves are symmetrical and their crests lie in vertical planes.

(v) *The airflow across a plateau*

By integrating the solution for a two-dimensional ridge (II, Eq. (11)) with respect to x we obtain the airflow up on to or down from a semi-infinite plateau. The formulae are

$$U\zeta/U_1 = a^2 \left\{ \tan^{-1}\frac{x}{b} \cos lz \mp \tfrac{1}{2} \log \frac{b^2}{b^2 + x^2} \sin lz \right\} \qquad . \quad . \quad (5)$$

$$\zeta_1 = a^2 \tan^{-1}\frac{x}{b} \qquad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

the upper and lower signs in (5) corresponding to $B = 0$ and $A = 0$ respectively.

For large positive or negative x the logarithm term becomes indefinitely large and the only practical solution is to omit it. This is equivalent to putting $A = B$, in accord with the suggestion at the end of (ii) above.

Alternatively we could argue that the perturbation method does not succeed because it leads to infinite displacements if $A \neq B$. But if we are content to reject the positive exponential in (2) when $k > l$ because it is not bounded, it seems legitimate to put $A = B$ in this case.

(vi) *The drag on a mountain*

The last section suggests a solution which is determined entirely by the ground shape. The behaviour at infinite height does not arise. We might insist, for instance, that the airstream should exert a thrust on the mountain in the direction of flow. The mean drag per unit length is seen, by considering the section $X_4 X_5$, to be

$$\frac{k}{2\pi} \int_0^{\frac{2\pi}{k}} p \frac{w}{U} dx = - U E \quad . \quad . \quad . \quad . \quad (7)$$

As we have seen, this is zero, in cases where $k > l$, because the wave crests lie in vertical planes so that, if there were an adiabatic layer at the bottom of the atmosphere, there could be no drag exerted by the ground, even if $k < l$ higher up. Consideration of drag is really irrelevant in perturbation theory because the stream with velocity U is supposed to be maintained by an influence not discussed – the large-scale pressure field.

If the pressure on the lee side of a hill were greater than at the crest we would expect separation of the flow and the formation of standing or moving eddies. This is often observed. If the pressure were less on the lee side we would expect large velocities down the lee slope, and this too is often observed. It seems that both cases are possible. The large velocities down the lee slope have generally been observed when lee waves form : for instance Helm Bars accompany the Helm Wind (Manley 1945); so that to have a pressure minimum on the lee side of a hill may require an airstream capable of containing lee waves.

(vii) *The importance of lee waves*

Lee waves are those which have zero amplitude at the ground, have a maximum at some middle level, and fade away higher up. The neighbouring wavelengths behave in a similar way except that they have finite amplitude at the ground and therefore only appear *over*, and not to the lee of, hills; they too have a maximum amplitude in middle levels and are therefore of great interest, many of their properties are similar to those of lee waves. Waves of much shorter length have a maximum amplitude at the ground and are therefore less interesting in the study of air motion produced in the upper air by flow over mountains.

Waves of much longer length than lee waves present the problem discussed in this section. There is no reason to suppose that the motion is necessarily of the type described by perturbation theory, and the fact that we cannot obtain a solution that dies away upwards is good reason for supposing otherwise.

The behaviour of lee waves is representative of the behaviour of the band of wavelengths that is important, and therefore merits special attention.

(viii) *The possibility of a solution*

It has been supposed that any given airstream can find a way of flowing over an obstacle in the manner prescribed by the linearised equations. Dr. R. R. Long, during

a recent conference,* questioned this hypothesis. He pointed out that, in laboratory experiments, if certain fluid streams were blocked by an obstacle a solitary wave like a hydraulic jump travelled upstream in such a way as to modify the whole of the oncoming current. Furthermore, in many cases, a jump occurs on the lee side and the stream passing away downstream is not the same as the one coming from upstream. Professor Queney has recently (privately during the International Conference at Toronto) expressed a similar view – that we are not justified in expecting to find a solution and that the flow is not of the kind supposed.

(ix) *The behaviour of lee waves*

In I and II attention was mainly given to the properties of lee waves. Dr. Long pointed out that waves of finite amplitude could not progress indefinitely without change of form. In the atmosphere it is now known that regions of turbulent motion sometimes occur around the crests of waves and it may be that the dissipation thereby produced prevents any further distortion of the wave as it proceeds through the air.

Even if the flow is not exactly like that supposed in the linearized theory for all wavelengths then the behaviour of the lee waves is probably not very different. This belief was fortified by Förchtgott's observations (described by Scorer 1952) which tended to show that only when conditions were favourable for lee waves was the flow laminar. In other cases the flow separated from the ground and vertical overturning occurred.

For this reason we shall study what kinds of airstream can contain these waves, and compare different airstreams that can contain waves of the same length. In cases where lee waves are not possible we do not at present know what the flow is like for certain, though the observations of Förchtgott and of Long (1953) in connection with the ' Bishop wave ' phenomenon give some enlightenment.

(x) *Mother-of-pearl clouds*

If we accept the last section, and say that only waves that are ' contained by the airstream ' (those for which negative exponential solution in the uppermost layer is possible) can occur we must explain how mother-of-pearl clouds can be produced orographically.

Because these clouds do not always turn into ice clouds their temperature is probably just above $-40°C$ in which case they are probably in the lower part of the ozonosphere, which is known to be lowered in cases of intense cyclonic activity so that such a temperature can reasonably be supposed to occur at heights around 26 km.

It is often argued that the tropopause, because it is under a layer of air of high static stability, acts as a lid to wave motions. This is not true for waves of the length under discussion (where the earth's rotation is unimportant) because l^2 is increased as β is increased and so k exceeds l by a smaller amount in the stratosphere than in the troposphere. A decrease of wind above the tropopause has the same effect. If there were a sufficiently deep layer of zero stability ($\beta = 0$, $l^2 = 0$) then $k > l$ for all k, and $A = 0$ and waves of all lengths would be decreased in amplitude with height. It has been argued by Scorer (1953a) that the upper half of the ozonosphere is such a layer for in it convection is active. Thus, provided that the wind has a component across the mountains at all heights, so that the disturbance due to the mountains can be communicated to those great heights, there is no reason why a disturbance of the kind supposed in the perturbation theory should not occur.

* Symposium on ' The use of models in geophysical fluid dynamics,' held at Johns Hopkins University, September 1953.

(xi) *Flow over an isolated hill*

The above discussion arises because neither of the two possible solutions of (1) decreases as $z \to \infty$. The mountain ridge has been assumed to be two-dimensional. We shall now see that if it is isolated both solutions decrease in amplitude with height. This does not avoid a choice having to be made.

From II we have, for flow across the ridge whose height is

$$\zeta_1 = \frac{a^2 b}{b^2 + x^2} = a^2 \int_0^\infty e^{-kb+ikx}\, dk \quad \text{(real part)} \qquad (8)$$

the disturbance at height z is

$$\zeta = \frac{U_1}{U} a^2 \int_0^\infty e^{-kb+ikx-\mu z}\, dk$$

$$\simeq \frac{U_1}{U} a^2 \frac{b + ix}{b^2 + x^2} e^{-ilz} \quad \text{(real part)} \qquad (9)$$

This corresponds to the solution $A = 0$. That for $B = 0$ is obtained by reversing the sign of l. If the airstream makes angle ϕ with the line normal to the crest of the ridge and polar coordinates (r, θ) are used, writing

$$X = r \cos(\theta + \phi), \quad Y = r \sin(\theta + \phi) \qquad (10)$$

we have

$$\zeta_1(\phi) = \frac{a^2 b}{b^2 + X^2} \qquad (11)$$

$$\zeta(\phi) = \frac{U_1}{U} a^2 \frac{b + iX}{b^2 + X^2} e^{-ilz \sec\phi} \qquad (12)$$

$$= \frac{U_1}{U} a^2 (b^2 + X^2)^{-\frac{1}{2}} e^{ix} \qquad (13)$$

where $x = T - lz \sec\phi$, and $T = \tan^{-1}\frac{X}{b}$. A solitary hill may be represented by the integral

$$\zeta_1 = \int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} \Phi(\phi) \frac{a^2 b}{b^2 + X^2}\, d\phi \qquad (14)$$

the hill being imagined as composed of a set of ridges at various angles ϕ all passing through the origin and Φ being the relative intensity of the component at angle ϕ. The corresponding disturbance at height z is therefore

$$\zeta = \int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} \Phi \frac{U_1}{U} a^2 (b^2 + X^2)^{-\frac{1}{2}} e^{ix}\, d\phi \qquad (15)$$

Since $x \to -\infty$ as $\phi \to \pm\frac{\pi}{2}$ the integral is highly oscillatory at the ends of the range of integration and is therefore suitable for evaluation by the principle of stationary phase. In such case

$$\zeta = \Phi \frac{U_1}{U} a^2 (b^2 + X^2)^{-\frac{1}{2}} \left(\frac{2\pi}{|x''|}\right)^{\frac{1}{2}} \cos\left(x \pm \frac{\pi}{4}\right) \qquad (16)$$

evaluated for the value of ϕ which satisfies

$$\partial x/\partial\phi = 0 \qquad (17)$$

where $x'' = \partial^2 x/\partial\phi^2$, and the sign in the argument is the sign of x''. Since x contains z linearly so does x'' and so for large z

$$\zeta \propto z^{-\frac{1}{2}} \quad . \quad . \quad . \quad . \quad . \quad (18)$$

This result has already been quoted by Scorer (1952). It means that arguments which reject a solution on the grounds that it does not vanish for large z cannot be used for solitary hills.

3. Airstreams with lee waves

In order for lee waves to occur their amplitude must decrease to zero at great heights and be zero at the ground, which is presumed level on the lee side of the hill. Even if the ground is not level, large amplitude vertical motion away from a hill on the leeside is still produced by lee waves whose phase and amplitude may however be modified by other hills.

The waves in the lee of the standard ridge given by (8), and from which any desired profile may theoretically be built up, are (II, Eqs. (26) and (33))

$$2\,i\,a^2\,\pi\,e^{-kb+ikx}\,\frac{U_1\,w_{z,k}}{U_z\,\partial w_{1,k}/\partial k} \quad \text{(real part)} \quad . \quad . \quad . \quad (19)$$

At high levels either U_z must increase indefinitely, which is impossible, or $w_{z,k}$ must decrease asymptotically to zero upwards, in order that the waves shall not extend up to infinity. This means that at some level $w_{z,k}$ is increasing downwards with a curvature of the $w_{z,k}$ – profile as indicated in Fig. 2 for $z > 0$. If the slope of $w_{z,k}$ is continuous, which is true if β and U are continuous functions of z, the curvature must change sign at some level if $w_{z,k}$ is zero at the ground. The curvature is given by (1), which $w_{z,k}$ satisfies, and so since w'' must change sign before w, as z decreases, the coefficient $(l^2 - k^2)$ must change sign. This is why the l^2-profile is important.

There are many different l^2-profiles which can achieve very similar results and so, in order to give such guidance as the theory is capable of to those wishing to interpret actual profiles some examples are now presented.

In all examples l_s^2 is assumed to be constant in the uppermost layer, $z \geqslant 0$. In each of the first four cases l_i^2 is constant from the interface down to the ground at $z = -h$, and an inversion (discontinuity) of temperature is supposed to exist at the interface. The value of l_i^2 necessary for waves of a given length to be possible is calculated for four different magnitudes of the inversion. The amplitude of the waves in the four different cases is seen to be not very different so that airstreams with these four l^2-profiles can in some respects be described as nearly ' dynamically equivalent.' The profile of $w_{z,k}$ shows that the more intense the inversion the more pronounced is the maximum of lee wave amplitude at the inversion : this fact was noted by Yates (1949).

In II it was shown that if the airstream is sampled by a balloon which ascends through waves an ' incorrect ' temperature and wind profile is obtained. In the example there given a layer of nearly constant lapse rate was replaced in the soundings by two layers of steeper lapse with an inversion in between. One is tempted to hope that the profile obtained would be ' dynamically equivalent ' to the original one, but the hope is groundless for the profile obtained depends on the position of release and rate of ascent of the balloon, and the wavelength. It would be possible for the balloon to ascend from the ground through a wave trough, which is warmed, up to levels where the wave amplitude was very small : the only effect then would be a warming of the lower layers. Meanwhile the effect on the wind observations depends not on the vertical displacement of the streamlines, but on the variation of it with height. Inversions and shallow layers of high

wind revealed by balloon soundings may, however, often be taken as an indication that wave motion is present.

The calculations proceed as follows : In the upper layer

$$w_{s,k} = e^{-\mu_s z}, \quad \mu_s = +(k^2 - l_s^2)^{\frac{1}{2}} \qquad . \quad . \quad . \quad (20)$$

At the interface, it has been shown (Scorer 1951) Eqs. (20) and (17) that

$$\left.\begin{aligned} w_{i,k}'/w_{i,k} &= -\mu_s \frac{s^2}{1+\epsilon} + \epsilon \frac{g}{U_i^2} \\ \text{and} \qquad w_{i,k} &= w_{s,k}/s \end{aligned}\right\} \qquad . \quad . \quad . \quad (21)$$

where ϵ is the change of density at the interface, namely $(\rho_{0,i} - \rho_{0,s})/\rho_{0,i}$, and $s = U_s/U_i$. When $s \neq 1$ there is a discontinuity of velocity at the interface. In the lower layer

$$w_{i,k} = A \cos \nu_i z + B \sin \nu_i z, \quad \nu_i = +(l_i^2 - k^2)^{\frac{1}{2}} \qquad . \quad . \quad (22)$$

Applying the boundary conditions (21) at the interface, $z = 0$

$$A = 1/s, \quad B = \left(-\frac{\mu_s s^2}{1+\epsilon} + \frac{\epsilon g}{U_i^2}\right) \Big/ s\nu_i \qquad . \quad . \quad . \quad (23)$$

whence, at the ground at $z = -h$, using (25) below, we find that

$$\frac{\partial w_{1,k}}{\partial k} = \frac{k \sin \nu_i h}{s \nu_i^3} \left\{ h\nu_i^2 + \frac{s^2 \nu_i^2}{(1+\epsilon)\mu_s} + \left[h\left(-\frac{\mu_s s^2}{1+\epsilon} + \frac{\epsilon g}{U_i^2}\right) - 1 \right] \left[-\frac{\mu_s s^2}{1+\epsilon} + \frac{\epsilon g}{U_i^2} \right] \right\} \quad . \quad (24)$$

This last expression largely determines the amplitude of the lee waves. It is obviously not simply dependent upon any of the variables involved, though changes in s are seen to have least influence since s is almost certain to lie between about 0·8 and 1·2. In any case if $s \neq 1$ the interface is unstable for disturbances of small wavelength, while if $s = 1$ it is not unstable unless $\epsilon < 0$. We shall deal therefore with a stable airstream, and put $s = 1$ henceforth.

ϵ may vary from zero to about ·03 (for an 8°C inversion), ν_i^2 is positive if $l_i > k$ but may vary as far as $-k^2$ for an adiabatic layer ($l_i^2 = 0$). In all these examples we have kept k the same, but this is not the only way in which air streams could be compared. One might, for instance, compare various l^2-profiles which, with a given mountain, give the same maximum vertical component of velocity.

The comparisons most easily made are determined by the form of the lee-wave equation. This states that $w = 0$ at the ground, i.e.,

$$\nu_i \cot \nu_i h = -\frac{\mu_s s^2}{1+\epsilon} + \frac{\epsilon g}{U_i^2} \qquad . \quad . \quad . \quad (25)$$

From this, when k, h, μ_s, ϵ, U_i, and s are given it is easy to calculate ν_i and hence l_i. To calculate k and l_i from the amplitude is incomparably longer because of the complicated way in which k is involved in (19) via (22) and (24).

In all cases we use the kilometre as the unit of length and put $k = 1$ (lee-wavelength $= 2\pi$km), $h = 2$ km, $\mu_s = \frac{1}{2}$ km^{-1}, (i.e. $l_s^2 = \cdot 75$ km^{-2}), $U_i = 14{\cdot}1$ m sec^{-1}. It is then found that the relationship between ϵ and l_i is as given in Table 1.

In case 3, since $\nu_i = 0$, (22) is replaced by

$$w_{i,k} = A + Bz. \qquad . \quad . \quad . \quad . \quad (26)$$

The relative amplitudes of the lee waves produced by the same mountain in these found cases are shown in Fig. 1.

TABLE 1. CHARACTERSTICS OF LEE WAVES IN 4 AIRSTREAMS. THE WAVELENGTH IS THE SAME IN ALL CASES. THE LAST COLUMN GIVES THEIR RELATIVE EXCITATION BY A RIDGE.

Case No.	ϵ (%)	Approx. inversion (°C)	l_i^2	$(\partial w_{1,k}/\partial k)^{-1}$
1	0	0	2·04	0·125
2	1	2·7	1·79	0·197
3	2	5·4	1	0·255
4	3	8·0	0	0·098

If instead of a sharp inversion ($l^2 = \infty$ at the height of the inversion) there is a shallow middle layer in which l^2 is large, then airstreams equivalent to examples 1-4 in giving the same value of k, can be obtained as follows. The lower layer is divided into two layers denoted by suffixes i (upper) and j (lower), the upper one being of depth h', the lower of depth $h - h'$.

Putting $s = 1$, $\epsilon = 0$, we find, using (21) at both interfaces, that

$$w_{i,k} = \cos \nu_i z - \frac{\mu_s}{\nu_i} \sin \nu_i z \quad . \quad . \quad . \quad . \quad . \quad (27)$$

$$w_{j,k} = \frac{1}{\nu_i} (\nu_i \cos \nu_i h' + \mu_s \sin \nu_i h') \cos \nu_j (z + h') + \frac{1}{\nu_j} (\nu_i \sin \nu_i h' - \mu_s \cos \nu_i h') \sin \nu_j (z + h') \quad . \quad . \quad (28)$$

and the lee-wave equation, corresponding to (22), is

$$\nu_j \cot \nu_j (h - h') = \nu_i \frac{\nu_i \sin \nu_i h' - \mu_s \cos \nu_i h'}{\nu_i \cos \nu_i h' + \mu_s \sin \nu_i h'} \quad . \quad . \quad (29)$$

The left-hand side is replaced by $\mu_j \coth \mu_j (h - h')$ if $k > l_j$, where $\mu_j = +(k^2 - l_j^2)^{\frac{1}{2}} = +i\nu_j$.

TABLE 2. CHARACTERISTICS OF THREE AIRSTREAMS WHICH HAVE THE SAME LEE-WAVELENGTH (THE LAST COLUMN GIVES THE RELATIVE EXCITATION BY A RIDGE)

Case No.	h' (km)	l_i^2	ν_i	l_j^2	μ_j	$(\partial w_{1,k}/\partial k)^{-1}$
5	0·25	5	2	1·08	·29 i	0·156
6	0·37	5	2	0	1	0·129
7	0·65	3·25	1·5	0	1	0·151

Cases 5-7 in Table 2 show how, with the same lee-wave length, different thicknesses of a very stable middle layer correspond to different stabilities in the lower layer. It must be emphasized that the variety of airstreams all possessing the same lee-wave length is infinite, and that, though in some cases the wavelength is the most easily observed quantity, it may be changed by a factor of two or three without altering very obviously the nature of the flow. Also, it must be emphasized that all the cases presented here are particular. *No* general qualitative conclusion can be drawn without further argument. When the profile of l has been chosen there is an infinity of airstreams with that profile. We are still at liberty to choose either the velocity or temperature profile and the l-profile then determines the other. This means, among other things, that the amplitude of the

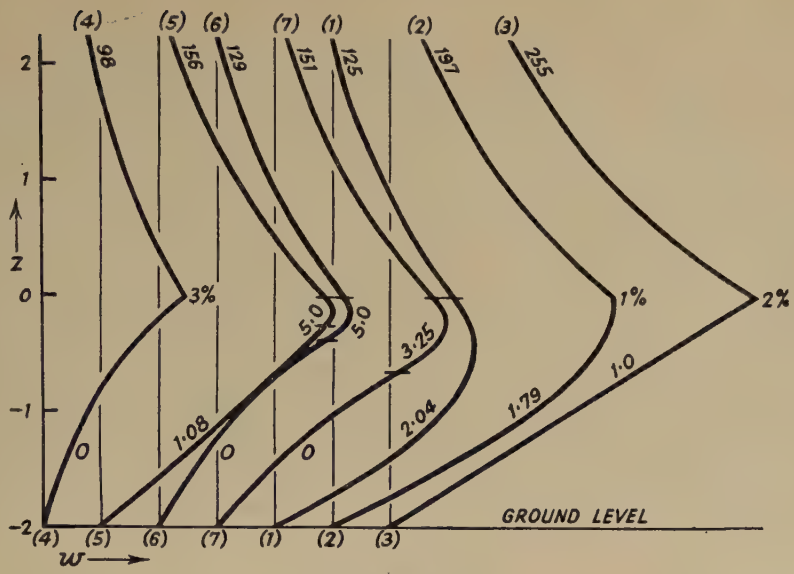


Figure 2. The curves indicate the variation with height of the vertical velocity in waves in the lee of the ridge given by (8), for seven different airstreams which all have the same lee (stationary) wavelength of 2π km. The zero is displaced so as to separate the curves : in all cases $w = 0$ at the ground.

The relative amplitude is indicated against the top of each curve, and the number of the case against the top and bottom in brackets. In all cases $l_s^2 = 0{\cdot}75$ km^{-2} for $z > 0$. For $0 \geqslant z \geqslant -2$ km (the ground is at -2 km) the value of l^2 is indicated against the curves. Where there is a discontinuity of density (cases 2, 3, 4) the percentage discontinuity is indicated : also $\partial w/\partial z$ is discontinuous there, and it is equivalent to a very thin layer in which l is very large. The curvature takes the same sign as $(k - l)$, and in these cases $k = 1$. The maximum of the wave amplitude is at or near to where l is largest.

waves set up by the standard ridge is not uniquely determined by the quantities so far calculated because there is a variety of values of U_1/U_z for a given profile of l. The quantity $(\partial w_{1,k}/\partial k)^{-1}$ only gives one component in the formula (19). In the three-layer case we find that

$$\begin{aligned}\frac{\nu_i^2\,\nu_j^2}{k}\frac{\partial w_{1,k}}{\partial k} = \cos\nu_i h' \Big\{ &\cos\nu_j (h-h') \Big[-\nu_j^2 - \nu_i^2 - \mu_s \nu_i^2 (h-h') - \nu_j^2 \mu_s h \Big] \\ &- \sin\nu_j (h-h') \Big[-\nu_i^2 \nu_j (h-h') + \mu_s \nu_j - \frac{\nu_i^2 \nu_j}{\mu_s} - h' \nu_j \nu_i^2 \Big] \Big\} \\ + \sin\nu_i h' \Big\{ &\cos\nu_j (h-h') \Big[-\mu_s \nu_i + \frac{\nu_j^2 \nu_i}{\mu_s} - \nu_i^3 (h-h') + h' \nu_i \nu_j^2 \Big] \\ &+ \sin\nu_j (h-h') \Big[\nu_i \nu_j (2 + \mu_s h) \Big] \Big\} \qquad . \quad . \quad . \quad (30)\end{aligned}$$

Since there are many ways of producing the same kind of wave motion it is not possible from observations, to conclude that certain features of the velocity or temperature profiles are necessary for waves to occur. Although in a particular locality a certain feature of the airstream, such as a strong inversion, may usually be found when waves occur this may simply be because the actual weather systems that occur do not produce other types of wave-bearing airstream at that place. One might argue that the limitations of the linearized equations make it necessary to seek formulae of prognostic value in an analysis of many observations, but there is no evidence that so long as the flow is laminar the conclusions of perturbation theory are much in error.

4. Conclusion

The unsettled problem of what solution should be chosen for the long wavelengths may be unreal : the perturbation solution may not be a practical one at all. This emphasizes the importance of lee waves and the band of wavelengths around them because of their large amplitude in middle levels. The properties of lee waves are similar to those of neighbouring wavelengths. The complexity of the mathematics prohibits the application of the formulae direct to actual cases, and in particular to forecasting. Some possibilities have been illustrated here, and with an understanding of them it will probably be possible to explain the behaviour of actual airstreams; also it is now clear that the profile of the quantity l^2, or of l, or of l^{-1}, is the feature that should be studied. In particular, lee waves can only exist if there is a layer low down in which l exceeds its value higher up, and the lee wavelength is such that k is equal to an intermediate value of l.

If l increases at higher levels wave motion is probably impossible because almost no waves are ' contained.' The flow is then either not steady, or not wavelike and there is overturning of some sort.

References

Benton, G. S. 1954 ' The occurrence of critical flow and hydraulic jumps in a multi-layered system,' *J. Met.*, **11**, p. 139.

Hoiland, E. 1952 *Proc. O.S.T.I.V.*, 1952 (in press).

Long, R. R. 1953 'A laboratory model resembling the "Bishop-wave" phenomenon,' *Tech. Rep. Civ. Eng., Johns Hopkins Univ.*, Baltimore, No. 3.

Lyra, G. 1943 ' Theorie der stationären leewellenströmung in freier atmosphäre, *Z. angew. Math. Mech.*, Berlin, **23**, p. 1.

Manley, G. 1945 ' The helm wind of Crossfell,' *Quart. J. R. Met. Soc.*, **71**, p. 197.

Palm, E. 1952 ' On the formation of surface waves in a fluid flowing over a corrugated bed and on the development of mountain waves,' *Astrophys. Norvegica*, **5**, No. 3.

Queney, P. 1947 ' The problem of airflow over mountains; a summary of theoretical studies,' *Bull. Amer. Met. Soc.*, **29**, p. 16.

1952 *Proc. O.S.T.I.V.*, 1952 (in press).

Scorer, R. S. I 1949 ' Theory of waves in the lee of mountains,' *Quart. J. R. Met. Soc.*, **75**, p. 41.

II 1953 ' Theory of airflow over mountains : II – The flow over a ridge,' *Ibid.*, **79**, p. 70.

1951 ' Gravity waves in the atmosphere,' *Arch. Met. Geoph. Biokl.* A, Vienna, **4**, p. 176.

1952 'Airflow over mountains,' *Sci. Progr.*, **40**, p. 466.

1953a 'Air motion in the upper atmosphere,' *Nature*, **172**, p. 138.

Wurtele, M. G. 1953 ' The initial-value lee-wave problem in an isothermal atmosphere,' unpublished manuscript.

Yates, A. H. 1949 ' Standing wave exploration by sailplane,' *Weather*, **4**, p. 40.

Zierep, J. 1952 ' Leewellen bei geschichteter Anströmung,' *Ber. Deutsch. Wetterd. U.S. Zone*, No. 35, p. 85.

551.551

A note on the two-dimensional equation of diffusion in the atmosphere

By D. R. DAVIES
Sheffield University

(Manuscript received 8 September 1953, in revised form 12 April 1954)

SUMMARY

In this paper the shearing advection term, suggested recently by H. Lettau, is introduced into the well-known two-dimensional equation formulated by Sutton and Calder in their treatment of turbulent diffusion, and a solution of the ensuing equation is obtained subject to the conditions prescribed by the problem of diffusion from an infinite continuous line source, situated at ground level and orientated in a direction normal to that of the mean wind velocity. The predicted values of cloud height and peak concentration at a distance of 100 m downwind of the source, when compared with previous theoretical values and available experimental results in *neutral stability conditions*, show that no significant change in theoretical values is obtained by the introduction of Lettau's term, and the order of magnitude of the deviation from the Calder prediction produced by this term is found to be the same *at all distances* downwind of the source. The shearing advection term may consequently be ignored *in the conditions investigated.*

1. INTRODUCTION

Solutions have been obtained, e.g., by Calder (1949) and Sutton (1949), of the two-dimensional equation of atmospheric diffusion subject to the conditions prescribed by a continuous infinite line source placed, at ground level, in a direction normal to that of the mean wind, and they are found to be in good agreement with experiment in certain conditions of atmospheric stability and roughness of ground. Lettau (1951), however, has recently suggested that an additional term must be introduced into the equation to allow for the mixing due directly to variations of mean speed of eddying volumes of air in passing from one level to another in the shearing turbulence of a boundary layer. He derives the expression $\alpha\, l^2 \frac{\partial \bar{u}}{\partial z}\frac{\partial \bar{x}}{\partial z}$, describing the rate of transfer due to this effect, across unit area of a plane which is normal to the mean wind direction and is moving with the mean speed of the fluid at the level considered. In this expression l denotes the vertical mixing length, $\bar{u}$ the mean wind speed, x the mean concentration at height z above ground level, and α a numerical proportionality factor.

Since Lettau's paper may not be easily accessible, we first give a brief description of the derivation of his term $\alpha\, l^2 \frac{\partial \bar{u}}{\partial z}\frac{\partial \bar{x}}{\partial z}$ and we then investigate quantitatively the influence of this term on the solution of the two-dimensional equation of diffusion.

The equation of continuity of matter is

$$\frac{\partial x}{\partial t} = -\nabla \,.\, \mathbf{V}x, \qquad (1)$$

where x and $\mathbf{V}$ denote the instantaneous values of concentration and velocity at a point in a turbulent flow field. If u, v, w denote the components of $\mathbf{V}$, measured relative to orthogonal axes Ox, Oy, Oz and we write $u = \bar{u} + u'$, $v = \bar{v} + v'$, $w = \bar{w} + w'$, $x = \bar{x} + x'$, where the bar operator has the conventional meaning, so that $\overline{x'} = 0$, $\overline{u'} = \overline{v'} = \overline{w'} = 0$, then, using the equation $\nabla \,.\, \mathbf{V} = 0$, we obtain in the usual manner the diffusion equation in the form

$$\frac{d\bar{x}}{dt} = -\left[\frac{\partial}{\partial x}(\overline{u'x'}) + \frac{\partial}{\partial y}(\overline{v'x'}) + \frac{\partial}{\partial z}(\overline{w'x'})\right] \qquad (2)$$

Lettau now introduces three quantities, x', y', z', having the dimensions of length and defined so that

$$x' = -\left[x'\frac{\partial \bar{x}}{\partial x} + y'\frac{\partial \bar{x}}{\partial y} + z'\frac{\partial \bar{x}}{\partial z}\right] \qquad (3)$$

where x', y', z' may be regarded as components of displacement of a turbulent element and $(\overline{x'^2})^{\frac{1}{2}}$, $(\overline{y'^2})^{\frac{1}{2}}$, $(\overline{z'^2})^{\frac{1}{2}}$ are clearly associated with mixing lengths in the x, y, z directions. The flux expressions in Eq. (2) may then be written in the usual form

$$\overline{u'x'} = -K_x \frac{\partial \bar{x}}{\partial x}, \quad \overline{v'x'} = -K_y \frac{\partial \bar{x}}{\partial y}, \quad \overline{w'x'} = -K_z \frac{\partial \bar{x}}{\partial z}, \qquad (4)$$

where K_x, K_y, K_z are the eddy diffusivities in the x, y, z directions and are given by the usual expressions

$$K_x = \overline{u'x'}, \quad K_y = \overline{v'y'}, \quad K_z = \overline{w'z'}, \qquad (5)$$

describing the increase of diffusing power with the scale of the diffusion process. Lettau points out that Eqs. (4) are valid provided

$$\overline{u'y'} = \overline{u'z'} = \overline{v'x'} = \overline{v'z'} = \overline{w'x'} = \overline{w'y'} = 0, \qquad (6)$$

i.e. if the turbulent flow is perfectly random.

Suppose, however, that the motion is constrained by the presence of a solid boundary along $z = 0$ and the mean wind is parallel to the boundary and in the Ox direction. In this case of shear-flow turbulence there exists a gradient, $\frac{\partial \bar{u}}{\partial z}$, of the mean velocity $\bar{u}$ and, following the usual mixing-length procedure, Lettau takes the u' fluctuation due to turbulent vertical displacement to be proportional to $z' \frac{\partial \bar{u}}{\partial z}$, so that the term $\overline{u'z'}$ may be written in the form $\alpha \overline{z'^2} \frac{\partial \bar{u}}{\partial z}$ or $\alpha l^2 \frac{\partial \bar{u}}{\partial z}$, where l denotes the mixing length and α is a proportionality factor. The flux of mass in the x-direction then becomes

$$\overline{u'x'} = -\left[\overline{u'x'} \frac{\partial \bar{x}}{\partial x} + \overline{u'z'} \frac{\partial \bar{x}}{\partial z}\right] = -\left[K_x \frac{\partial \bar{x}}{\partial x} + \alpha l^2 \frac{\partial \bar{u}}{\partial z} \frac{\partial \bar{x}}{\partial z}\right], \qquad (7)$$

the term involving $\overline{u'y'}$ being zero. The term $\alpha l^2 \frac{\partial \bar{u}}{\partial z} \frac{\partial \bar{x}}{\partial z}$ introduced thus by Lettau is referred to as the " shearing advection term " and clearly originates from the correlation between those u' and x' fluctuations brought about by turbulent vertical displacements, when $\bar{u}$ and $\bar{x}$ are functions of z. The flux of mass in the z direction is given by

$$\overline{w'x'} = -\overline{w'z'} \frac{\partial \bar{x}}{\partial z} = -K_z \frac{\partial \bar{x}}{\partial z},$$

since $\overline{w'x'} = \overline{w'y'} = 0$.

In problems involving two-dimensional diffusion, e.g., from a continuous uniform infinite line source, the relevant equation, according to Lettau, is consequently

$$\bar{u} \frac{\partial \bar{x}}{\partial x} = \frac{\partial}{\partial x}\left[K_x \frac{\partial \bar{x}}{\partial x} + \alpha l^2 \frac{\partial \bar{u}}{\partial z} \frac{\partial \bar{x}}{\partial z}\right] + \frac{\partial}{\partial z}\left[K_z \frac{\partial \bar{x}}{\partial z}\right], \qquad (8)$$

and is the form of the diffusion equation considered in this paper.

Lettau then suggests that the effect of this shearing advection term is to produce a coefficient of diffusion in the down-wind direction which increases with down-wind distance x from the source. This result, however, indicates only the first stage in demonstrating the possible importance of his term, and clearly what we need is a solution of the atmospheric diffusion equation with the new term incorporated. The change in prediction formula produced by Lettau's term will be a *direct* measure of its effect on two-dimensional diffusion theory.

It should also be noted that Lettau (1951, p. 440) relates the shearing advective term $l^2 \frac{\partial \bar{u}}{\partial z} \frac{\partial \bar{x}}{\partial z}$ to a term proportional to $\frac{\partial \bar{x}}{\partial x}$ by a transformation which does not appear to be valid in the context of diffusion of a cloud of particles. In Eq. 12 (a) of his paper Lettau identifies δu (obtained from $\delta u/\delta z$), the increase of wind speed due to an increase δz in vertical height above ground, with the increase (at a given time t) of particle velocity in the cluster in the direction of the mean wind; he writes δu (vertical increment) $= \delta x/t$, which does not appear to be permissible. We note that in treating the atmospheric diffusion equation, $\bar{u}$ is of course the *mean* wind speed, a function of z *only*. Moreover, we find that substitution of the solution $\bar{x}(x, z)$ of equation (8) into Lettau's relation between $l^2 \frac{\partial \bar{u}}{\partial z} \frac{\partial \bar{x}}{\partial z}$ and $\frac{\partial \bar{x}}{\partial x}$ (1951, equation 12 (a)) demonstrates that the relation is not valid.

The shearing advection effect expressed in the form $l^2 \frac{\partial \bar{u}}{\partial z} \frac{\partial \bar{x}}{\partial z}$ is, however, valid and may be important in practical cases; it is the aim of this note to determine *quantitatively* the significance of this term in the particular case of two-dimensional continuous diffusion, using the formulation of K_z suggested by Calder.

2. The two-dimensional equation of diffusion and boundary conditions

Referred to rectangular axes Ox and Oz, the former being at ground level and directed parallel to the mean wind, and the latter being directed vertically upwards, the two-dimensional equation of mean diffusion in the steady state is now

$$\bar{u} \frac{\partial \bar{x}}{\partial x} = \frac{\partial}{\partial x}\left(l^2 \frac{\partial \bar{u}}{\partial z} \frac{\partial \bar{x}}{\partial z}\right) + \frac{\partial}{\partial z}\left(K_z \frac{\partial \bar{x}}{\partial z}\right), \quad . \quad . \quad . \quad (9)$$

Lettau's expression having been included (with α taken to be unity following the usual mixing-length procedure) and K_x neglected as in the two-dimensional theories of Sutton and Calder for continuous diffusion problems.

Following previous work we write

$$\bar{U}/\bar{U}_1 = (z/z_1)^m \quad . \quad . \quad . \quad . \quad . \quad (10)$$

and

$$K_z = a_z \bar{U}_1{}^{1-n} z^{1-m}, \quad . \quad . \quad . \quad . \quad (11)$$

where $\bar{U}_1$ is the mean wind speed at height z_1, the coefficient $a_z = M \bar{U}_1{}^n z_1{}^{-m}$ (M being defined in Calder's paper), and $m = n/(2 - n)$. In neutral stability conditions it has been shown by Sheppard (1947) that a good approximation to the relation between l and z up to a height of about 1 m is obtained by writing

$$l = kz, \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

where k is von Karmàn's constant for the lower atmosphere. We now assume this relation is valid over the height of diffusion considered by Calder and Sutton, i.e. about 10 m, although it has been indicated by Sheppard that for greater heights a better approximation to l is obtained by using a power law in z with index slightly greater than unity. In the mathematical analysis discussed in the present paper it has been possible to consider only a linear relationship. However, it is not unreasonable to suppose that the consequent error will be small.

Substitution of equations (10), (11), and (12) into (9) leads to the equation

$$\frac{\partial \bar{x}}{\partial x} = (mk^2)\, z \frac{\partial^2 \bar{x}}{\partial x \partial z} + \frac{(a_z \bar{U}_1{}^{-n} z_1{}^m)}{z^m} \frac{\partial}{\partial z}\left(z^{1-m} \frac{\partial \bar{x}}{\partial z}\right) \quad . \quad . \quad (13)$$

We now write

$$z^{m+1/2} = \zeta, \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

and equation (13) becomes

$$\frac{\partial \bar{x}}{\partial x} = q_1 \zeta \frac{\partial}{\partial \zeta}\left(\frac{\partial \bar{x}}{\partial x}\right) + r_1 \zeta^{-s} \frac{\partial}{\partial \zeta}\left(\zeta^s \frac{\partial \bar{x}}{\partial \zeta}\right), \quad . \quad . \quad . \quad (15)$$

where we have written

$$m\,(m + 1/2)\, k^2 = q_1, \quad . \quad . \quad . \quad . \quad (16)$$

$$a_z\, (\bar{U}_1)^{-n} z_1{}^m (m + 1/2)^2 = r_1, \quad . \quad . \quad . \quad . \quad (17)$$

and

$$1/(1 + 2m) = s \quad . \quad . \quad . \quad . \quad (18)$$

The boundary conditions for a continuous infinite line source lying along $x = 0$, $z = 0$ are

$$\begin{aligned} &\bar{x} \to \infty, \quad \text{as} \quad x \to 0 \quad \text{for} \quad z = 0, \\ &\bar{x} \to 0\ , \quad \text{as} \quad x \to 0 \quad \text{for} \quad z > 0, \\ &\bar{x} \to 0\ , \quad \text{as} \quad x \to \infty, \\ &K_z \frac{\partial \bar{x}}{\partial z} \to 0\ , \quad \text{as} \quad z \to 0; \end{aligned} \quad . \quad . \quad . \quad (19)$$

with the continuity condition that the rate of transfer of $\bar{x}$ across unit width of *any* plane normal to the x axis is given by

$$\int_0^\infty \bar{u}\,\bar{x}\,dz = Q, \quad . \quad . \quad . \quad . \quad . \quad (20)$$

the constant rate of emission per unit length of the source.

3. Solution of the diffusion equation

We now seek solutions of the form

$$\bar{x} = A\,x^{n_0} f(u) \quad . \quad . \quad . \quad . \quad . \quad (21)$$

where

$$u = \zeta^\beta / z^q \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

and A, n_0, β, q are to be determined. From (15), (21), (22) we obtain the equation

$$n_0\,x^{n_0-1} f - q\,x^{n_0-1}\,u f' = q_1\,\beta\,x^{n_0-1}\,[(n_0 - q)\,u f' - q\,u^2 f''] + \\ + r_1\,x^{n_0-2q/\beta}\,[\beta\,s\,u^{1-2/\beta} f' + \beta\,(\beta-1)\,u^{1-2/\beta} f' + \beta^2\,u^{2-2/\beta} f''] \quad . \quad (23)$$

The x variable cancels throughout, if we write

$$q/\beta = 1/2 \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

and equation (23) becomes tractable if we write $\beta = 2$, $q = 1$, and we then have to obtain a suitable particular solution of the equation

$$(4r_1\,u - 2q_1\,u^2) f'' + [2r_1\,(1+s) + (1 + 2q_1\,n_0 - 2q_1)\,u] f' - n_0 f = 0 \quad . \quad (25)$$

We first determine the value of n_0 to satisfy the continuity condition (20). Substitution of (21) and (22) into (20) gives

$$A\,\overline{U}_1\,(1+2m)^{-1}\,z_1^{-m}\,x^{[n_0+q\beta^{-1}+(1+2m)^{-1}\,q\beta^{-1}]} \int_0^\infty f(u)\,u^{-m(1+2m)^{-1}}\,du = Q.$$

The left-hand side of this equation is to be independent of x and hence, using equation (24), we obtain

$$n_0 = -(1+m)/(1+2m) \quad . \quad . \quad . \quad . \quad (26)$$

This result was, of course, obtained by Calder and is in close agreement with experimental observations in neutral stability conditions. It is clear that any solution of the form (21), using result (24) and the continuity condition, leads to expression (26).

It now appears that a solution of equation (25) exists in the form of a special case of a hypergeometric function.

We write

$$(q_1\,u)/(2\,r_1) = t \quad . \quad . \quad . \quad . \quad . \quad (27)$$

Equation (25) becomes

$$t\,(1-t)\,\frac{d^2 f}{dt^2} + \left\{ -n_0 - \left[-\frac{(2q_1\,n_0 + 1)}{2q_1} + 1 \right] t \right\} \frac{df}{dt} - \frac{n_0}{2q_1} f = 0 \quad . \quad (28)$$

a solution of which is expressible as a hypergeometric function $F(a_n, b_n; c_n; t)$ where

$$a_n + b_n = -(2q_1\,n_0 + 1)/(2q_1),$$

$$a_n\,b_n = +n_0/(2q_1),$$

and

$$c_n = -n_0.$$

We find that $a_n = c_n$ and $b_n = -1/(2q_1)$, and when $t < 1$, which is found to be easily satisfied in the diffusion problem to be considered,

$$f = F = (1-t)^{-b_n} \\ = [1 - q_1\,(2r_1)^{-1}\,z^{1+2m}\,x^{-1}]^{1/(2q_1)} \quad . \quad . \quad . \quad (29)$$

A particular solution of the diffusion equation (13) satisfying conditions (19) and (20) is thus seen to be

$$\bar{x} = A\, x^{-(1+m)/(1+2m)} \left[1 - q_1 (2r_1)^{-1} z^{1+2m} x^{-1}\right]^{1/(2q_1)} \quad . \quad . \quad (30)$$

where

$$A = Q(1 + 2m)\, z_1{}^m (\bar{U}_1 I)^{-1}, \quad . \quad . \quad . \quad . \quad (31)$$

and

$$I = \int_0^\infty f(u)\, u^{-m/(1+2m)}\, du.$$

In the practical cases considered the concentration is found to be effectively zero for values of t very considerably less than 1 and the contribution to the integral in the range 1 to ∞ in t is then found to be vanishingly small. We consequently write, for mathematical convenience, using (27) and (29)

$$I = (2r_1/q_1)^{(1+m)/(1+2m)} \int_0^1 (1 - t)^{1/(2q_1)}\, t^{-m/(1+2m)}\, dt$$

$$= \frac{(2r_1/q_1)^{(1+m)/(1+2m)}\, \Gamma\{1 + (2q_1)^{-1}\}\, \Gamma\{(1 + m)/(1 + 2m)\}}{\Gamma\{1 + (2q_1)^{-1} + (1 + m)/(1 + 2m)\}} \quad . \quad . \quad (32)$$

4. Numerical evaluation of the effect of Lettau's term in the two-dimensional equation of diffusion

Using the data given by Calder (1949) for a long-grass surface, the theoretical height of smoke cloud, at a distance of, say, 100 m downwind of a continuous infinite line source, may now be computed from equation (30) in order to evaluate the effect of Lettau's term on the Sutton-Calder solutions. The height of cloud is taken, for practical purposes, to be the point above ground, at a prescribed down-wind distance, x_1, from the source, at which the concentration is $\frac{1}{10}$ the value of the concentration on $z = 0$ at the same down-wind distance. The theoretical cloud height, H, is easily found from equation (30) to be given by the expression

$$H = \{2r_1\, x_1\, q_1{}^{-1} \left[1 - 10^{-2q_1}\right]\}^{1/(1+2m)}. \quad . \quad . \quad . \quad (33)$$

The appropriate numerical values of q_1 and r_1 are obtained from the observed mean wind velocity profile, and are given in Table 1, together with the numerical values of the other physical entities employed in the calculation. In Table 2 the numerical results obtained from the analysis are displayed.

TABLE 1. Numerical values of the physical entities (cm sec units) employed in the calculation

$Q = 1$ g/sec	$m = 0{\cdot}205$	$z_0 = 3$
$q' = 4{\cdot}42$	$d = 30$	$\bar{U}_1 = 500$
$z_1 = 200$	$r_1 = 0{\cdot}195$	$q_1 = 0{\cdot}023$

TABLE 2. Theoretical and experimental results giving cloud height and peak concentration at a distance of 100 m downwind of a continuous infinite line source, positioned at ground level, and orientated normally to the mean wind direction, in neutral stability conditions over a long-grass surface

	Calculated		Observed
Type of theoretical basis	Calder solution	Calder solution with Lettau term introduced	
Height of cloud (m)	10·50	10·0	10·10
Concentration (mg/m^3) at $x = 100$ m, $z = 0$	3,540	3,610	3,560

The numerical values for cloud height and peak concentration shown in Table 2 indicate clearly that no significant change in prediction formula is obtained by the introduction of Lettau's term $l^2 \frac{\partial \bar{u}}{\partial z} \frac{\partial \bar{x}}{\partial z}$, with $l = kz$, in the *two-dimensional, continuous* diffusion problem considered and in the conditions of *neutral stability* investigated. It is important to note that the order of magnitude of the deviation from the Calder prediction produced by Lettau's term is found to be the same *at all distances* down-wind of the source. Finally, it should be added that Lettau's term may well be significant for instantaneous sources, but the mathematical treatment in these cases presents difficulties which have not yet been resolved.

References

Calder, K. L.	1949	*Quart. J. Mech. Appl. Math.*, **2**, p. 153.
Lettau, H.	1951	*M.I.T. Symposium, Geophys. Res. Pap.*, No. 19, p. 437.
Sheppard, P. A.	1947	*Proc. Roy. Soc.* A, **188**, p. 208.
Sutton, O. G.	1949	*Quart. J. R. Met. Soc.*, **75**, p. 333.

Discussion

Dr. H. Lettau (*communicated*) : I have been privileged to see Dr. Davies's paper in course of publication and suggest there is one specific point needing clarification.

Dr. Davies states that owing to continuity the rate of transfer of matter across unit width of any plane normal to the x axis is

$$Q = \int_0^\infty \bar{u}\,\bar{x}\,dz = \text{const.} \qquad (20)$$

where Q = source strength per unit length of the constant line source. The above Eq. (20) is used to determine a parameter n_0 in Eq. (25). Now, the continuity condition is, first of all, plainly the basis of the diffusion equation. This fact is expressed by Eqs. (1), (2), and also (8) provided steady state exists, and unidimensional but height-varying wind can be assumed. When ordinary eddy diffusion in the x direction is neglected in comparison with the advection term, that is, the term $\partial\,[K_x\,\partial\bar{x}/\partial x]/\partial x$ is small compared to $\bar{u}\,\partial\bar{x}/\partial x = \partial\,(\bar{u}\,\bar{x})/\partial x$, and shearing advection is disregarded, Eq. (20) is justifiable because the integration of the diffusion equation with respect to z from zero to infinity disposes of eddy diffusion in the z direction, whereupon Eq. (20) expresses simply the Gaussian divergence theorem. However, when Eq. (9) is assumed to hold true and $\partial\bar{u}/\partial z \neq 0$, one must consider that continuity gives, after making a development similar to that above,

$$Q = \int_0^\infty [\bar{u}\,\bar{x} + l^2\,(\partial\bar{u}/\partial z)\,(\partial\bar{x}/\partial z)]\,dz = \text{const.} \qquad (20\text{A})$$

For fluid layers in which $l\,\partial\bar{u}/\partial z \simeq \text{const} = (\tau_0/\rho)^{\frac{1}{2}}$ = friction velocity of the wind profile (τ_0 = ground drag) and, furthermore, $l \simeq k\,z$ the relative magnitude of the second term of the integrand is readily estimated. Although the second term may be approximately $\frac{1}{10}$ of the first, it must be expected that the parameter n_0 determined with the aid of Eq. (20A) will turn out to be different from the form derived by Calder (compare Eq. (26) and subsequent discussion). Also, the deviation from the Calder prediction produced by the shearing advection term may come out differently.

A more general point is that, *a priori*, shearing advection effects will always be least significant in problems where diffusing matter is emitted from infinite area or line sources, especially when they act continuously. For instantaneous sources, especially when matter is released at constant rate only during a limited interval of time from a finite line or area source, the shearing advection effect may be significant. Difficulties of mathematical treatment can possibly be reduced by the investigation of genuine molecular diffusion of matter in a fluid having a laminar velocity profile.

551.511 : 526.8

A note on the use of the stereographic map projection in dynamical meteorology

By T. H. ELLISON
Clare College, Cambridge

(Manuscript received 18 November 1953, in revised form 4 May 1954)

SUMMARY

The equations of motion in curvilinear coordinates, in which the curvature of the earth is taken into account, are shown to be quite simple when the coordinate system is a particular one, closely related to the stereographic map projection. As an illustration of the use of these coordinates, derivations are given of the vorticity equation and of Ertel's Theorem.

1. INTRODUCTION

It is common to neglect the curvature of the earth's surface in deriving the equations of motion for the atmosphere. This procedure does not lead to results in violent disagreement with observation, but it is desirable to be able to estimate precisely the error which results in any particular application of the equations. McVittie (1948) has derived the general equations for motion on a spherical earth, and has discussed coordinate systems for dynamical meteorology (1951). However, his work is concerned with the mathematical rather than the practical aspects of the problem. In this note the equations of motion, including any effect of the curvature of the earth, are derived for a particular coordinate system and are found to be quite simple. To illustrate the application of the equations, useful forms of the vorticity equation and of Ertel's Theorem are deduced.

2. THE EQUATIONS OF MOTION

In rectangular cartesian coordinates which are rotating with a constant angular velocity $\boldsymbol{\omega}$, the equations of motion of a frictionless fluid can be written

$$\frac{D\mathbf{u}}{Dt} + \frac{1}{\rho}\nabla p + \nabla\Phi - 2\mathbf{u} \times \boldsymbol{\omega} = 0 \qquad . \qquad . \qquad . \qquad (1)$$

where $\frac{D}{Dt} = \frac{\partial}{\partial t} + (\mathbf{u} \,.\, \nabla)$ and Φ is the observed geopotential (including the effects of both true gravity and the centrifugal force). We shall neglect tidal forces, so the surface of the earth may be considered a geopotential.

Since the atmosphere is very shallow it is convenient to describe the position of a point, Q, in it by one coordinate, such as the height, which measures its distance from the surface, and two describing the position of the point P on the surface immediately beneath Q; these two coordinates may be taken as the rectangular cartesian coordinates of the point P′ which represents P on some map.

It is simplest to use the stereographic projection for the map. This is illustrated in Fig. 1. A point P on the earth's surface is projected into P′ on the tangent plane TP′ from C which is the antipode of T. This projection is orthomorphic so that any small area on the earth has the same shape on the map. Thus rectangular cartesian coordinates (x, y) for P′ can be used as orthogonal coordinates for P. To describe the position of a general point in the atmosphere Q, we use these coordinates for P and its height, PQ $= z$. The three coordinates of Q, (x, y, z), form an orthogonal system.

The local scale, s, or ratio of a small length on the map to the corresponding horizontal length in the atmosphere can easily be shown to be given by

$$s = \frac{4E^2 + x^2 + y^2}{4E^2} \cdot \frac{E}{E + z} \qquad . \qquad . \qquad . \qquad . \qquad (2)$$

The coordinates of Q in a rectangular cartesian system, (x_1, y_1, z_1) with origin at O and z-axis along OT then are

$$x_1 = s^{-1}x;\ y_1 = s^{-1}y;\ z_1 = E + z - (x^2 + y^2)/2Es \qquad . \qquad . \qquad (3)$$

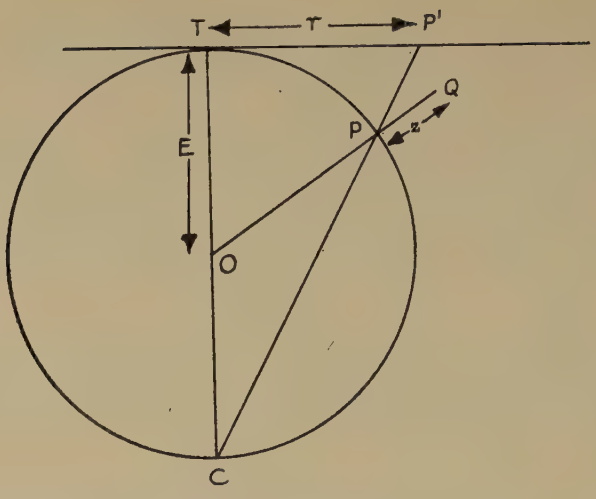


Figure 1. The stereographic map projection. $r^2 = x^2 + y^2$.

Now G. B. Jeffery (1915) has shown how to write down the equations of motion in general orthogonal curvilinear coordinates. If we apply his result to our system of coordinates, we obtain the following equation for u, where (u, v, w) are the components of velocity in the local x-, y-, and z-directions (i.e., the actual components of velocity which would be measured by an anemometer, $u = s^{-1}\frac{Dx}{Dt}$), ϕ is the latitude and ψ the angle between the local x-direction and east:

$$\frac{Du}{Dt} - uv\frac{\partial s}{\partial y} + v^2\frac{\partial s}{\partial x} - uw\frac{\partial \ln s}{\partial z} + \frac{s}{\rho}\frac{\partial p}{\partial x} + s\frac{\partial \Phi}{\partial x} - 2\omega(v\sin\phi - w\cos\phi\cos\psi) = 0 \quad . \quad . \quad . \quad (4)$$

where

$$\frac{D}{Dt} = \frac{\partial}{\partial t} + su\frac{\partial}{\partial x} + sv\frac{\partial}{\partial y} + w\frac{\partial}{\partial z}$$

The equation for v may be written down by symmetry. We do not require the corresponding equation for w since it is known that, so far as large-scale motion is concerned, the atmosphere may be treated as in hydrostatic equilibrium. Thus

$$\frac{\partial \Phi}{\partial z} + \frac{1}{\rho}\frac{\partial p}{\partial z} = 0 \quad . \quad . \quad . \quad . \quad . \quad (5)$$

The continuity equation is

$$\frac{D\ln\rho}{Dt} = -s^2\left(\frac{\partial us^{-1}}{\partial x} + \frac{\partial vs^{-1}}{\partial y} + \frac{\partial ws^{-2}}{\partial z}\right) \quad . \quad . \quad . \quad (6)$$

For practical purposes it is permissible to omit from Eq. (4) the small terms containing w, and, since the *vertical* scale of atmospheric motion is small compared with the radius of the earth, to replace s by σ which is defined by

$$\sigma = \frac{4E^2 + x^2 + y^2}{4E^2} \quad . \quad . \quad . \quad . \quad (7)$$

When this is done and when, following Sutcliffe (1947), the height z is replaced as vertical coordinate by the pressure p, we have

$$\frac{Du}{Dt} - uv\left(\frac{\partial\sigma}{\partial y}\right)_p + v^2\left(\frac{\partial\sigma}{\partial x}\right)_p + \sigma\left(\frac{\partial\Phi}{\partial x}\right)_p - 2\omega v\sin\phi = 0 \quad . \quad . \quad (8)$$

with

$$\frac{D}{Dt} = \left(\frac{\partial}{\partial t}\right)_p + \sigma u\left(\frac{\partial}{\partial x}\right)_p + \sigma v\left(\frac{\partial}{\partial y}\right)_p + \varpi\frac{\partial}{\partial p} \quad . \quad . \quad . \quad (9)$$

where $\varpi = \dfrac{Dp}{Dt}$

The continuity equation now becomes

$$-\sigma^2\left[\left(\frac{\partial u\sigma^{-1}}{\partial x}\right)_p + \left(\frac{\partial v\sigma^{-1}}{\partial y}\right)_p\right] = \frac{\partial \varpi}{\partial p} \quad . \quad . \quad . \quad (10)$$

3. The vorticity equation

The vertical component of vorticity in our coordinates is (see Jeffery 1915),

$$\zeta = \sigma^2\left(\frac{\partial v\sigma^{-1}}{\partial x}\right)_z - \sigma^2\left(\frac{\partial u\sigma^{-1}}{\partial y}\right)_z \quad . \quad . \quad . \quad . \quad (11)$$

but we shall work with the related quantity called the ' isobaric vorticity ' which is used by Sutcliffe. This is defined by an equation similar to (11) but with the derivates at constant pressure instead of constant height, thus

$$\zeta_p = \sigma^2\left(\frac{\partial v\sigma^{-1}}{\partial x}\right)_p - \sigma^2\left(\frac{\partial u\sigma^{-1}}{\partial y}\right)_p \quad . \quad . \quad . \quad . \quad (12)$$

Let $F = \sigma^{-2} f = 2\sigma^{-2}\omega \sin\phi$ and $Z = \sigma^{-2}\zeta_p$, then on subtracting $\left(\dfrac{\partial}{\partial y}\right)_p$ of (8) from $\left(\dfrac{\partial}{\partial x}\right)_p$ of the similar equation for v, and performing some simple algebra we obtain

$$\frac{D(Z+F)}{Dt} + (Z+F)\left[\left(\frac{\partial u\sigma}{\partial x}\right)_p + \left(\frac{\partial v\sigma}{\partial y}\right)_p\right] + \frac{\partial v\sigma^{-1}}{\partial p}\frac{\partial \varpi}{\partial x} - \frac{\partial u\sigma^{-1}}{\partial p}\frac{\partial \varpi}{\partial y} = 0 \quad . \quad (13)$$

The divergence can be eliminated by (10) so that

$$\frac{D(Z+F)}{Dt} + (Z+F)\left[\sigma^{-2}\frac{D\sigma^2}{Dt} - \frac{\partial \varpi}{\partial p}\right] + \frac{\partial v\sigma^{-1}}{\partial p}\frac{\partial \varpi}{\partial x} - \frac{\partial u\sigma^{-1}}{\partial p}\frac{\partial \varpi}{\partial y} = 0 \quad . \quad (14)$$

Thus
$$\frac{D(\zeta_p + f)}{Dt} - (\zeta_p + f)\frac{\partial \varpi}{\partial p} = -\sigma\left(\frac{\partial v}{\partial p}\frac{\partial \varpi}{\partial x} - \frac{\partial u}{\partial p}\frac{\partial \varpi}{\partial y}\right) \quad . \quad . \quad . \quad . \quad (15)$$

which is the vorticity equation in our coordinates. The isobaric vorticity can easily be estimated from synoptic charts since in the geostrophic approximation

$$\zeta_g = F^{-1}\left[\left(\frac{\partial^2 \Phi}{\partial x^2}\right)_p + \left(\frac{\partial^2 \Phi}{\partial y^2}\right)_p\right]$$

TABLE 1. Values of scale factor and related quantities for stereographic projection centred on north pole

ϕ (°N)	r (10^3 km)	σ	σ^2	f (10^{-5} sec^{-1})	F (10^{-5} sec^{-1})
90	0	1	1	14·6	14·6
70	2·25	1·03	1·06	13·7	12·9
50	4·64	1·13	1·28	11·2	8·8
30	7·35	1·33	1·77	7·29	4·1
10	10·69	1·70	2·89	2·53	0·87

4. Ertel's theorem

Ertel (1942) has proved the interesting theorem that in any adiabatic motion the scalar product of the gradient of potential temperature with the absolute vorticity divided by the density is constant. The physical background of this theorem has been discussed more recently by Batchelor (1952) in the course of his work on the effect of turbulence on material lines and surfaces.

In the form stated above, Ertel's theorem remains true if the potential temperature is replaced by any other conserved quantity; in the modified form which we prove below that is no longer the case. We are concerned not with the complete scalar product but only with the quantity $(\zeta + f)\frac{\partial\theta}{\partial p}$.

First observe that since for adiabatic motion $\frac{D\theta}{Dt} = 0$,

$$\frac{D}{Dt}\left(\frac{\partial\theta}{\partial p}\right) = -\left\{\frac{\partial u\sigma}{\partial p}\left(\frac{\partial\theta}{\partial x}\right)_p + \frac{\partial v\sigma}{\partial p}\left(\frac{\partial\theta}{\partial y}\right)_p + \frac{\partial\varpi}{\partial p}\frac{\partial\theta}{\partial p}\right\} \quad . \quad . \quad (16)$$

In the geostrophic approximation the thermal wind equation gives

$$\frac{\partial u\sigma}{\partial p}\left(\frac{\partial\theta}{\partial x}\right)_p + \frac{\partial v\sigma}{\partial p}\left(\frac{\partial\theta}{\partial y}\right)_p = 0 \quad . \quad . \quad . \quad . \quad (17)$$

so approximately

$$\frac{D}{Dt}\left(\ln\frac{\partial\theta}{\partial p}\right) = -\frac{\partial\varpi}{\partial p} \quad . \quad . \quad . \quad . \quad (18)$$

which is an interesting equation in its own right. When it is combined with (15) and the right-hand side of (15) neglected, we have

$$\frac{D}{Dt}\left\{(\zeta_p + f)\frac{\partial\theta}{\partial p}\right\} = 0 \quad . \quad . \quad . \quad . \quad . \quad (19)$$

which is a form of Ertel's law which can readily be applied in practice.

References

Batchelor, G. K.	1952	*Proc. Roy. Soc.* A, **213**, p. 349.
Ertel, H.	1942	*Met. Z.*, **59**, p. 277.
Jeffery, G. B.	1915	*Phil. Mag.* 6th series, **29**, p. 445.
McVittie, G. C.	1948	*Quart. J. Mech. App. Math.*, Brown Univ., U.S.A., **1**, p. 174.
	1951	*J. Met.*, **8**, p. 161.
Sutcliffe, R. C.	1947	*Quart. J. R. Met. Soc.*, **73**, p. 370.

551.556.551.46

Wind stress on a water surface

By J. R. D. FRANCIS
Imperial College, London

(Manuscript received 26 January 1954, in revised form 9 April 1954)

Summary

Values for the shear stress τ_0 of the wind on a water surface, found in seven later experiments, are compared with the data of Francis (1951). Four sets of the new data show that the stress coefficient $C = \tau_0/\rho u^2$ is not constant, but that it increases somewhat with windspeed u (ρ = air density). Three sets merely show a large experimental scatter of C, and do not support any consistent law of drag. A theory is also discussed that τ_0 is mainly caused by the drag of the small ripples, and not by the drag of the big waves.

1. Introduction

There have been many attempts in the past to measure the tangential stress, τ_0, of the wind on a water surface such as the sea. In 1951 I published an account of some wind-tunnel experiments designed to give this stress, and a summary of the results of previous measurements.

I drew the conclusion from my work that the shear-stress coefficient $C = \tau_0/\rho u^2$ (ρ = air density, u = windspeed) was not a constant, but increased nearly linearly with u; and that the stress is caused mainly, if not wholly, by the form drag of the small-scale ripples superimposed on the big waves.

There have now been seven further attempts to find this important meteorological and oceanographical quantity. Unfortunately, however, the several experimenters have not presented their results on a common basis, or compared them fully with previous work, and it seems timely to make such a comparison.

τ_0 is usually measured by observing the slope of the water surface of an enclosed lake, pond or wind tunnel, when a wind is blowing over it. The slope i of a length of the water surface is determined by both τ_0 and also τ_b, the stress of the lake bottom on the water which is returning from the lee end to the weather end, underneath the upper wind drift current.

Thus, $\tau_0 + \tau_b = g\,\rho_1\,d\,i$ (ρ_1 = water density, d = mean depth).

To obtain a value of τ_0 it is therefore necessary to know τ_b/τ_0. Assuming laminar flow, or a constant eddy viscosity with depth, it can be shown that $\tau_b/\tau_0 = 0{\cdot}50$. Neither of these assumptions is likely to be valid in a lake or the sea, which are always turbulent; Francis (1953) shows that τ_b/τ_0 may be as low as 0·015, and that τ_b may probably be neglected in shallow, well-mixed lakes and wind tunnels. In deeper lakes or the ocean, thermal stratification may occur and is usually persistent, so that the wind-driven water motions are confined to the uppermost layer; the effective depth is therefore smaller than the actual depth of water, a correction which, applied to the above formula, has the same numerical effect as using a large τ_b/τ_0 ratio. In fact, it is experimentally difficult to measure i on deep lakes and oceans, so that the above complication rarely arises. In the following review, the values of C are all deduced on the common assumption that $\tau_b = 0$. In this case $\tau_0 = g\,\rho_1\,d\,i$ and $c = \tau_0/\rho u^2$.

2. Experiments in wind tunnels

Keulegan (1951) has made experiments rather similar to mine in a longer and wider wind tunnel of about the same depth. The waves caused by the wind on the longer fetch are larger; being in the same depth of water they would seem to be controlled more by the bottom than

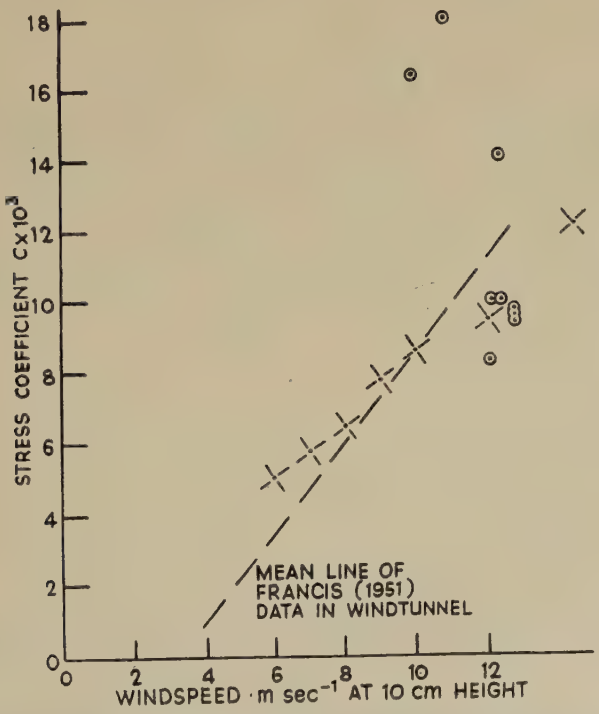


Figure 1. Wind stress on water. Small-scale wind-tunnel experiments. Crosses indicate Keulegan (1951) mean values, and circles Johnson and Rice (1952) values.

in my experiments, and so be less like deep water waves. Nevertheless, Keulegan's values of C, plotted in Fig. 1, fall close to mine, showing a steady increase of C with u.

On the other hand, the data of Johnson and Rice (1952), whose experiments were done in a wind tunnel similar to Keulegan's but with much deeper water, when plotted on the same axes (Fig. 1) give a larger spread of C for a much smaller range of u. It is not clear from the account of their work if any differences of method exist compared with those of Keulegan and myself.

Hamada, Mitsuyasu and Hase (1953) have also done laboratory experiments in a larger wind tunnel than mine. They compute τ_0 by a quite different method using the changes in the air velocity profile along the tunnel to find the rate of change of momentum in the boundary layer. This change, corrected for the variation of static pressure along the tunnel is claimed to be the shear force exerted by the air on the water surface. There must be some doubt as to the validity of the method, for it tacitly assumes that neither the roof nor the sides of the tunnel modify the growth of the air boundary layer on the water surface. The air passage was nearly square in section, a shape notorious for producing complicated flow conditions. Hamada *et al.* also observed the surface slopes i, which are compared with a computed slope i_{calc} obtained from the τ_0 values, using the relation $i_{calc} = 3\tau_0/(2\,\rho_1\,g\,d)$. This relation makes the constant eddy viscosity assumption that $\tau_b = 0{\cdot}50\,\tau_0$. There is a fairly good agreement between i and i_{calc}, showing that on the assumptions made, τ_0 measured by the surface slope method would have been the same as that measured by the boundary layer method.

However, as I have already discussed in Section 1, the constant eddy viscosity assumption is somewhat doubtful, and a coefficient 1·0 is more appropriate than 3/2 in the slope equation. To compensate for this change and still to keep the numerical agreement between i and i_{calc}, I have increased each of Hamada's τ_0 and C values by 50 per cent, and plotted them in Fig. 2. This is tantamount to assuming that there is an error of 50 per cent due to the sides and roof of the tunnel when τ_0 is found by the boundary layer method; and that τ_0 is correct when found from the water slope i, with coefficient 1·0 in the slope equation. It will be seen from the graph that C increases nearly linearly with u; there is no evidence for a constant value of C. The slope of the graph cannot be compared directly with the other laboratory data, for both C and u are based on the maximum windspeed at a particular section, and not on the speed at a specific height. This maximum will be affected to some extent by the roof of the tunnel, and not only by the water surface.

3. Experiments on lakes

Recently, a set of results has been published referring to conditions upon an intermediate scale between wind tunnel and the sea. Van Dorn (1952, 1953) has measured the surface gradient of a pond some 240 m long, 60 m wide and 2 m deep exposed to ocean winds. His waves are therefore probably subject to about the same bottom effect as mine, for though the pond is deeper than my wind tunnel, his waves are longer because of the greater fetch. His wind slopes i, converted to C, are referred to the 10-m windspeed, and must therefore be compared with my wind-tunnel results extrapolated to the 10 m condition; it will be remembered that my results thus extrapolated were typical of the data obtained over the sea by other workers. Fig. 3 shows the Van Dorn

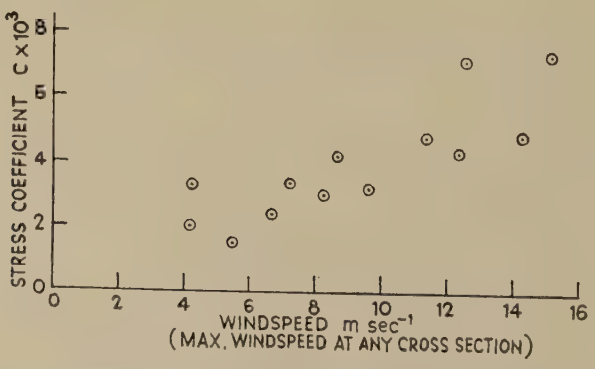


Figure 2. Wind stress on water. Wind-tunnel data of Hamada *et al.* (1953). Both C and u are based on the maximum wind speed in the tunnel, and not on the speed at any specific height above water level.

data, giving an increase of C with u, the values being quite close to mine which are the result of a large extrapolation of the small-scale tunnel measurements, the slope of the mean line consequently being tentative. Van Dorn's data are clearly more reliable.

There are two accounts of attempts to measure the wind stress on larger lakes. Keulegan (1953) publishes some old data on the slopes induced on the water levels in Lake Erie (about 390 km by 80 km and 18 m mean depth). His values of C, corrected to comply with my assumption that $\tau_b = 0$ (see Section 1) are plotted on Fig. 4, together with the curve of C against u proposed by Neumann (1948), which Keulegan uses as a comparison. It will be seen that there is a great scatter in the Lake Erie C values, and that they conform to Neumann's curve no more than to my straight line. The scatter shows how difficult it is to measure the wind stress by this method; high winds rarely last long enough to ensure that the water has reached a steady state; and at low winds the slopes are too small to measure accurately as they may be masked by the surges and seiche movements nearly always present. In this respect Neumann's proposal that τ_0 is proportional to $u^{1\cdot5}$, or C proportional to $u^{-0\cdot5}$, is somewhat doubtful, for most of the evidence of such a law depends on the values of C at low windspeeds. If only the values at higher windspeeds are considered then, within the limits of experimental error, the evidence is only of a nearly constant value of C.

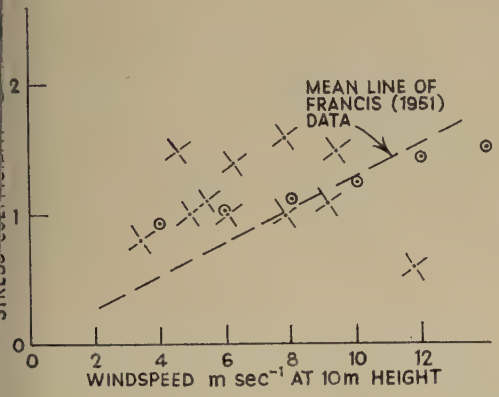


Figure 3. Wind stress on water. Large-scale experiments. Crosses Sheppard and Omar (1952); circles Van Dorn (1952) mean values.

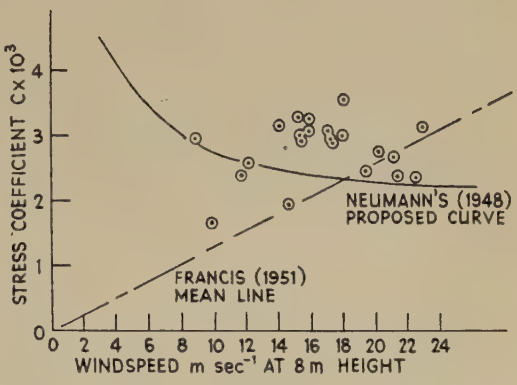


Figure 4. Wind stress on water. Keulegan's (1953) data from Lake Erie.

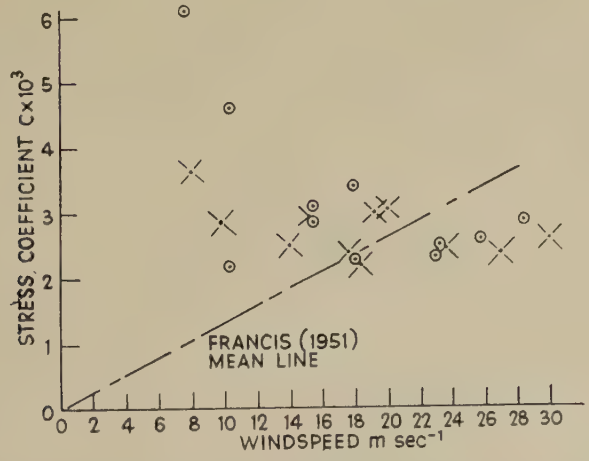


Figure 5. Wind stress on water. Hellström's (1953) data from Ringköbing Fiord. Circles indicate values based on isobar-spacing windspeeds, crosses values based on anemometer windspeeds.

The other account of slope observations on a lake is by Hellström (1953) who put six recording level gauges around Ringköbing Fiord (28 km by 11 km and 1·7 m deep). His data are plotted in Fig. 5, as two different sets of C values, one based upon the anemometer wind-speed, the other on the isobar spacings on the daily weather map. It will be seen that except for two high values of C at low windspeeds, there is little evidence for any variation of C with u. Hellström himself shows that $\tau_0 \propto u^{1\cdot8}$ (i.e., a decreasing value of C with u) but he uses a 'selected computation value' of u which is not always the arithmetical mean of the anemometer and weather-chart values. There is no reason given for such arbitrary selection except that the winds were initially reported in the Beaufort scale, and reconverted to a m/sec scale for his investigation. No doubt a certain amount of the scatter can be explained by this process.

4. Stress obtained from balloon ascents over the sea

A completely different method of finding τ_0 is by measuring the ageostrophic component of wind normal to the surface wind, using pilot balloons. The method, explained by Sutcliffe (1936), has been used by Sheppard and Omar (1952), using balloon ascents in the Trade-Wind areas. These authors admit the tentative nature of their results, which depend on a rather precise knowledge of the direction and magnitude of the geostrophic wind, and of the height of maximum wind speed; neither part of the information was well defined by the observations available. The computed values of C are plotted on Fig. 3, and show, broadly, a constant C as u increases. Priestley (1953), commenting on Sheppard and Omar's work, remarks that the method does not allow for a possible flux of momentum through the level of maximum wind speed; such a transfer might well be caused by buoyancy or other forces, and Priestley shows that it might be a function of windspeed, with the effect of increasing C as u increases.

It therefore appears that there is a possibility that the Sheppard and Omar results show a positive dC/du in the same way as was found in my wind-tunnel experiments, but more precise data are necessary to solve the problem satisfactorily.

5. The ripples on the large waves as the cause of wind stress

I have suggested (Francis 1951) that τ_0 is mainly caused by the form drag of the small, slow moving ripples and wavelets. Van Dorn's work lends some support to this view, for his C values are about the same as mine and are on a fetch some 30 times longer : the larger waves which must have been present in his experiments do not affect C much. The remaining roughness elements, the ripples, seem therefore to be the active ones in causing drag. The same conclusion can be drawn from a computation of the drag per unit area produced by waves of different sizes at the same windspeed, assuming that each wave acts, so far as the air is concerned, as a solid element travelling at wave speed V. Thus the drag per wave is proportional to $(u - V)^2 H$, where H is wave height, and the drag per unit area is proportional to $(u - V)^2 H/L$, where L = wavelength. A selection of wavelengths with their respective contributions to the total drag is given in Table 1, and it will be seen that small waves and ripples produce most of the total drag, because there are more of them, and they have a greater relative velocity $(u - V)$. The same coefficient of drag is used for all waves; this is likely to underestimate the contribution

TABLE 1. Theoretical contributions to the total wind stress by the form drag of waves of different sizes

Wavelength L (cm)	L/H	Wave speed V* (cm/sec)	Relative drag $(u - V)^2 H/L$ for 3 values of u		
			u (m/sec) 5	10	15
3	7	24	3·2	13·6	31
5	7	30	3·15	13·5	31
10	7	39	3·05	13·2	30·5
30	8	68	2·3	10·8	25·5
100	12	125	1·15	6·4	15·8
500	14	280	0·35	3·7	10·6
2,000	16	560	negative	1·2	5·5
10,000	20	1,250	negative	negative	0·31

* Calculated from perfect-fluid wave theory, $V^2 = gL/2\pi$.

rom the small waves, which are steeper and have a more angular crest than the large waves. Table 1 does not show a stress law in which C increases with u, because for simplicity it was ssumed that ripples are always present in the same numbers and sizes, whatever the windspeed; n fact, it is a common observation that such ripples appear more frequently and are more promin-ent in higher winds.

A complete proof of the ripple theory of drag demands a quantitative demonstration that he size and frequency of ripples increases with the windspeed, and that the separation of the effects of waves of different sizes is justified in what is probably a non-linear problem.

6. Conclusion

To sum up : the additional evidence of Keulegan (1951), Van Dorn (1953), Hamada *et al.* (1953) and, to some extent, Sheppard and Omar (1952) if Priestley's (1953) comments are valid, all tend to confirm my 1951 suggestion that C is not a constant but that it increases somewhat as u increases. The wind-tunnel data of Keulegan and Hamada *et al.* show C increasing nearly linearly with u, and these data alone confirm my tentative proposal that $C = 0{\cdot}00013\,u$ (C is non-dimensional, u is in m/sec). The remaining data reviewed above (Keulegan (1953), Johnson and Rice (1952), Hellström (1953)) merely show the large scatter which is usual in dealing with such a complex natural phenomenon; they do not support any other consistent stress law, e.g., Neumann's $C \propto u^{-0{\cdot}5}$, and Munk's $\tau_0 \propto u^2$ with a discontinuity of C at about 6·5 m/sec.

The weight of evidence would thus appear slightly to favour values of C increasing with u (and therefore τ_0 proportional to a power of u higher than the square). There is no more certain evidence for the traditional square law, $\tau_0 \propto u^2$ (e.g., C = constant); but the linear relation $C \propto u$ (e.g., the cube law $\tau_0 \propto u^3$) is only confirmed for wind-tunnel experiments.

It is, however, quite possible that these stress laws discussed above only hold in an intermediate range of windspeeds : in low winds, when there are either no waves at all, or the waves are of the non-rippled smooth-sided variety, a different law may be valid; and in very high winds, when the surface is torn adrift in a welter of spray, the flow conditions may be quite changed, perhaps being somewhat analogous to the flow of dry sand in a desert sandstorm.

7. Acknowledgment

I am much beholden to Professor P. A. Sheppard and to Professor R. B. Montgomery, who read, criticized, and improved the manuscript.

References

Francis, J. R. D.	1951	*Proc. Roy. Soc.* A, **206**, p. 387.
	1953	*J. Mar. Res.*, **12**, p. 93.
Hamada, T., Mitsuyasu, H. and Hase, N.	1953	*Transportation Tech. Res. Inst. Rep.*, Tokyo, No. 8.
Hellström, B.	1953	*Trans. Amer. Geo. Un.*, **34**, p. 194.
Johnson, J. W. and Rice, E. K.	1952	*Ibid.*, **33**, p. 845.
Keulegan, G. H.	1951	*J. Res. Bur. Stand.*, Washington, **46**, p. 358.
	1953	*Ibid.*, **50**, p. 95.
Neumann, G.	1948	*Z. Met.*, **2**, p. 193.
Priestley, C. H. B.	1953	*Quart. J. R. Met. Soc.*, **79**, p. 303.
Sheppard, P. A. and Omar, M. H.	1952	*Ibid.*, **78**, p. 583.
Sutcliffe, R. C.	1936	*Ibid.*, **62**, p. 3.
Van Dorn, W.	1952	*Scripps Inst. Ocean. Rep.* No. 52-60.
	1953	*J. Mar. Res.*, **12**, p. 249.

551.578.42 (427.2)

Snowfall probabilities on Merseyside

By G. REYNOLDS
Liverpool Observatory and Tidal Institute

(Manuscript received 19 February 1954, in revised form 20 March 1954)

Summary

The probabilities of snowfall and snow cover on Merseyside are examined by computing adjusted normal frequency distributions for 34 years of record at Bidston, Wirral, about three miles from the centre of Liverpool. A graph has been drawn from which the chances of these occurrences can be read off directly for any selected date or period. Estimates are also advanced of the probabilities of deep snow lying on the ground. These have been computed principally from records of heavy daily rainfall.

1. Introduction

The technical literature relating to snowfall and snow cover over the British Isles is not very extensive. Manley (1939) has discussed the relationship between snow cover and monthly mean temperatures, and more recently the annual reports of the Snow Survey of Great Britain have given factual evidence concerning post-war winters. Ashmore (1952) has provided valuable comparisons of the reliability of observations from the different types of record. With the exception of the last, which is of general application, greater stress has been laid upon the upland districts where the fall and cover is more extensive. In the more industrialized areas, the inconvenience and loss due to snow is much greater, however, owing in part, at least, to its rarity and therefore a lack of preparedness for tackling the problem when it arises.

In view of the dislocation caused to industry and communications by heavy snowfall, it will be appreciated that accurate knowledge of snowfall probabilities would be of great value to industrialists, municipal undertakings, Electricity, Gas and Water Boards, the Railway Executive and many others, enabling them to assess costs and insurable risks.

2. Snowfall and snow-cover probabilities

The purpose of this paper is to provide such information for Merseyside. To this end, data relating to the number of days of snowfall, and days with snow-lying on the ground at 09 hr for the 34 years, 1920-53, were extracted from the records maintained by the Liverpool Observatory and Tidal Institute, Bidston, and are shown in Table 1. In passing it may be mentioned that the average annual number of days of snow cover is 6·5, while the computed value, based on monthly mean temperature and derived from the graph in Manley's paper (1939) is 6·4, a very close agreement.

TABLE 1. Number of days of snowfall and snow-lying at 09 hr, 1920-53

	J.	F.	M.	A.	M.	J.	J.	A.	S.	O.	N.	D.
Snow	136	93	94·	32	9	0	0	0	1	9	37	79
Snow-lying	70	74	36	1	1	0	0	0	0	0	6	34

From these data, crude probabilities of snowfall and snow-lying in each winter month were evaluated. These probabilities are distributed approximately normally through the season. Accordingly with a view to improving the graduation, a normal frequency curve was fitted to the data of Table 1, allowance being made for the varying lengths of the months. On computing the first four moments, it was found possible to further improve the snowfall graduation by allowing for kurtosis (the distortion from the form of the normal frequency curve due to a disproportionally large or small number of observations in the intermediate ranges between mean and extreme

alues), and the snow-lying graduation by correcting for both slight negative skewness and urtosis (Brooks and Carruthers 1953). The skewness of the snowfall distribution was not ignificantly different from zero.

The isolated occurrence of snowfall in September might appear to be an error of observation n view of the fact that no other local station reported it (Gregory 1953). However, recomputation of the frequency graduation omitting this figure gives a curve whose goodness of fit is inferior o that using the complete record.

A case can also be advanced for omitting one or both of the occasions of snow-lying in April nd May, on the grounds that they represent a fall occurring just prior to 09 hr, which would probably only just cover the ground and would not impede traffic or interrupt communications, nd, in any case, very likely disappeared soon after the hour of observation. To investigate his, the snow-lying graduation was examined omitting both these occasions, and also omitting he May observation only. Considerable improvement to the goodness of fit of the curve was effected, but there was nothing to choose between the alternative omissions. Therefore, on the grounds of leaving out as little data as possible, the graduation using all the observations excepting he single May reading was accepted.

The parameters of the various distributions investigated are detailed in Table 2, in which μ_2, μ_3 and μ_4 are the second, third and fourth moments (the units of which are months of 30 days), γ_1 is the coefficient of skewness, and γ_2 is the degree of kurtosis.

TABLE 2. PARAMETERS OF THE FREQUENCY DISTRIBUTIONS OF SNOWFALL AND SNOW-LYING AT BIDSTON, 1920-53

	Moments			Skewness	Kurtosis
	μ_2	μ_3	μ_4	γ_1	γ_2
Snowfall	2·16	− 0·10	11·82	− 0·03	− 0·46
Snowfall (omitting Sept.)	2·14	− 0·04	11·50	0	− 0·50
Snow-lying	1·02	− 0·17	2·89	− 0·16	− 0·23
Snow-lying (omitting Apr. and May)	0·95	− 0·30	2·05	− 0·32	− 0·72
Snow-lying (omitting May)	0·99	− 0·28	2·35	− 0·28	− 0·60

The completed probability graduations are presented in Fig. 1, where the ordinates represent the probability of occurrence on any particular day and the dates are plotted as abscissa. The upper curve relates to snowfall and the lower curve to snow covering the ground. The use of these curves can be extended to estimations of expectations in a period of any length.

3. HEAVY FALLS

Turning to discussion of the probabilities of heavy snowfalls, records of the maximum depth occurring in each year for the period 1929-Feb. 1954, give a mean maximum depth of 2·7 in. with a standard deviation of 2·58 in. Attempts to fit a Poisson frequency distribution to the data met with failure owing to the larger number of deep falls than was derived theoretically.

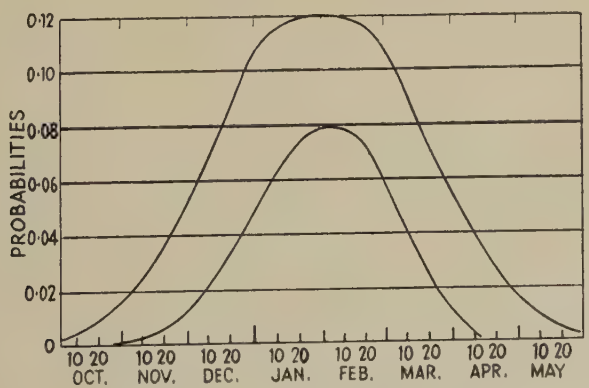


Figure 1. Probabilities of snowfall (upper curve) and snow-lying (lower curve) on Merseyside.

Closer scrutiny of the observations showed that they were not complete, and despite every effort to check them, the best list which could be compiled bore every evidence of being a biased record, with the recent years, 1947-54, well represented, and earlier years sparsely represented. No observations at all of snow depths were available for 10 of the 17 years immediately prior to 1945. It would be unwise to consider the years for which records were available as random samples from the whole population, as they bore every evidence of being biased towards the heavier falls. The only safe conclusion which could be reached was that falls of at least 6 in. deep have occurred four times, and only four times, in the last 26 years.

The indirect approach by way of heavy rainfall totals in a day, for which complete records were available since 1937, was therefore taken. Records of the number of occasions of daily rainfall within certain limits are given in Table 3.

TABLE 3. Occasions of heavy daily rainfall at Bidston, 1937-53

Month	No. of occasions of rainfall within limits (in.)					
	0·50–0·74	0·75–0·99	1·00–1·24	1·25–1·49	1·50 or more	Total
October	9	5	1	–	–	15
November	18	4	2	–	–	24
December	7	2	1	–	1	11
January	13	1	3	–	–	17
February	5	3	2	1	–	11
March	8	–	1	–	–	9
Total	60	15	10	1	1	87

Smoothed data were prepared from Table 3, retaining the main outlines, but adjusting, for example, the sequence of the total column from 15, 24, 11, 17, 11 and 9 to 15, 17, 18, 17, 11, 9. This smoothing is justified on the grounds that with such a short series of observations, some irregularities are bound to appear, and these are best removed before examining the data further. The January frequencies are another obvious example of this point. In support, it may be mentioned that over the period, 1937-53, the November rainfall was 16 per cent above, and the December rainfall was 14 per cent below, the long-period average.

The smoothed values were multiplied by a factor calculated by multiplying the mean snowfall probabilities (read off Fig. 1) by the number of days in the month and dividing the result by the average number of rain-days in the month (data taken from Gregory 1953). This factor converted the snowfall probabilities on any day to probabilities on any day of rain, and application of it to the smoothed values assumed that on any day when snow fell, all the precipitation took this form, and the temperature was such that it all lay on the ground. This is at variance with the facts and gave much too high a probability of heavy snowfall, so an appropriate reduction factor (s) had to be determined. At this stage, the resultant table gave a total frequency of snow depths greater than 6 in. of 11·60 per year. Now the observations show only four such depths in 26 years, so :

$$s = \frac{4}{26 \times 11{\cdot}60} = 0{\cdot}0133 \quad . \quad . \quad . \quad . \quad (1)$$

Multiplying the table of probabilities through by 0·0133 gave the final probabilities of Table 4.

It cannot be emphasized too strongly that s represents not a discrepancy between theory and observation, but is merely the magnitude of the constant conversion factor from rainfall to snow depth.

The author realises that the hypothesis that occurrences of heavy snowfall bear a direct linear relationship to occurrences of heavy rainfall involves an assumption which is yet unproven, but it seems a reasonable one to make. The alternatives that heavy snowfall and heavy rainfall are entirely independent, or inversely proportional to each other are unreasonable. The other alternative that the relationship between them is direct but complex remains, but with our present knowledge it seems best to accept the simpler relationship until it can be proven false.

In confirmation of the correctness of Table 4, it may be mentioned that one of the four depths observed as greater than 6 in. was greater than 9 in. which is the proportion to be expected from the table.

TABLE 4. MONTHLY PROBABILITIES OF DEEP SNOW

Month	6 to 9 in.	9 to 12 in.	12 to 15 in.	15 to 18 in.	18 in. and more
October	0·0020	0·0006	0·0002	0·0001	–
November	0·0096	0·0028	0·0012	0·0003	0·0002
December	0·0230	0·0064	0·0026	0·0007	0·0004
January	0·0296	0·0083	0·0035	0·0010	0·0005
February	0·0256	0·0074	0·0030	0·0010	0·0004
March	0·0160	0·0046	0·0018	0·0005	0·0003
All Winter	0·106	0·030	0·012	0·004	0·002

As to the absolute maximum snow depth possible in the area, rainfall greater than 1·5 in. in 24 hr has occurred during the months from October to March, on four occasions in the last 87 years. In addition, on 7-8 Feb. 1946, 2·16 in. of rain fell in 27 hr of continuous precipitation. Snow two feet deep appears, therefore, to be possible on Merseyside, but the chance of its occurrence is about once in a thousand years.

4. CONCLUSIONS

A method of calculating snowfall and snow-cover expectations has been advanced. This method is capable of being extended to other lowland areas as required, using principally the data tabulated in the *Monthly Weather Report*. In exposed moorland or mountainous districts, mass drifting would probably render it invalid, as the greater part of the dislocation could be caused by localised heavy drifts, not representative of the general snow cover. The data presented with regard to expected snow depths are rather more tentative, but provide the best information obtainable with present records.

REFERENCES

Ashmore, S. E. 1952 *Quart. J. R. Met. Soc.*, **78**, p. 629.

Brooks, C. E. P. and Carruthers, N. 1953 *Handbook of Statistical Methods in Meteorology*, London (H.M.S.O.).

Gregory, S. 1953 *A Scientific Survey of Merseyside*, Liverpool University Press.

Manley, G. 1939 *Quart. J. R. Met. Soc.*, **65**, p. 2.

551.508.54

A simple mechanism to improve the sensitivity of a Dines anemograph

By E. TROLLUND A.

Director, Meteorological Office of Chile, Santiago

(Manuscript received 30 December 1953, in revised form 20 April 1954)

SUMMARY

The motion of the brass rod which connects the float to the recording pen of the Dines anemograph is controlled by three ball bearings instead of the usual collar. The sensitivity of the instrument to small changes in wind speed and the reproducibility of the zero line have been much improved by this change.

The Meteorological Office of Chile has, installed in its Central Office, a Dines anemograph which was manufactured by a German firm, Steffen Heddes, and has been in operation more or less regularly since 1930. After its original installation the instrument functioned well for a number of years but, later, certain defects appeared which could no longer be corrected by ordinary adjustments to the apparatus. The difficulty was with the registering of the wind velocity; the pen became sluggish and could not be adjusted to its zero position. This reference

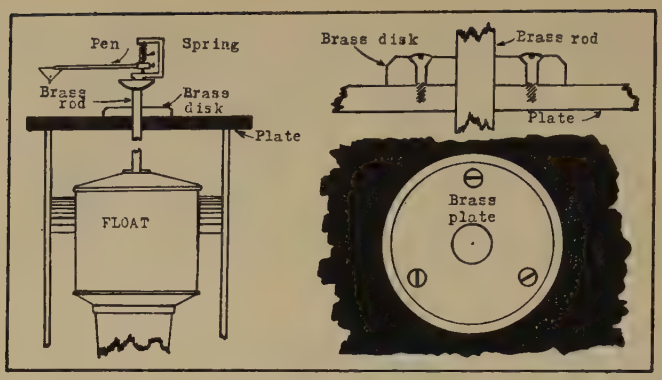


Figure 1. General construction of float, rod and pen.

line thus practically lost its value and the pen returned to it with great difficulty, usually remaining either above or below the line. Fig. 1 shows the general construction of the float and rod which carries the inscribing pen. The manufacturers recommend that no oil be used on the brass rod, the only lubrication being provided by the application to it of very small quantities of powdered graphite.

As illustrated in Fig. 1, the circular disc of brass is perforated in the centre, the perforation being of such a diameter that the brass rod bearing the inscribing pen can move freely in a vertical direction. The inner walls of the perforation are vertical and, with the repeated movement of the rod within it during a number of years, the friction of the parts, and especially the action of the sharp edges of the upper surface of the plate, result in a wearing away of the rod. This continued action causes a loss of smoothness in the movement of the rod. Furthermore, to ensure that the inscribing pen maintains the same pressure against the paper, the instrument is provided with a small spring located on a vertical shaft added to the upper part of the primary vertical rod. This spring, shown in Fig. 1, is weak, but it produces a horizontal pressure which forces the vertical rod always to tend to press against the same side of the perforation in the brass disk, thus greatly increasing the friction and wearing-away of the metal. This, of course, causes the rod to lose its verticality, which is of prime importance and, as the pressure of the spring is not always the same because of the various adjustments made by the several observers, the effects of friction are also variable.

The resultant effect of these several factors is that an irregular variation in the operation of the pen occurs, which notably reduces the efficiency of the recording. The author has prepared a simple bearing for the vertical brass rod which completely eliminates these difficulties and which may be of interest to other observatories using the same type of anemograph.

Instead of having the vertical brass rod supported in its motion by the entire surface of the side walls of the perforation made in the brass disk, it is arranged that it makes contact at only three points, which are sufficient to centre a cylindrical bar. The friction can be reduced to insignificant proportions by using three small steel ball-bearings to support the vertical brass rod. The three ball-bearings are mounted in small bars of brass, and these, in turn, are attached symmetrically to the original brass disks in such a manner that the vertical brass rod is supported at three points by the ball-bearings. Fig. 2 shows the entire support after its revision, and Fig. 3 indicates the details of the brass bars and ball-bearings. The ball-bearings move with absolute freedom, as is to be expected, and offer a negligible amount of friction to the vertical motion of the brass rod.

The method of mounting the ball-bearings is as follows. A hole of a diameter slightly greater than that of the ball is made at the end of the bar. The depth of this hole must be a little greater than the radius of the ball, which is then introduced into the cavity. By gently knocking around the hole with a small hammer as is seen in Fig. 4 the ball is retained in it but is able to rotate freely. This last condition is a very important one for the success of this device.

The brass bars, themselves, are attached to the disk by means of a small flat-headed screw. A cut has been made in each, as indicated in Fig. 3, which allows them to be adjusted so that neither too much nor too little pressure is exercised on the vertical rod.

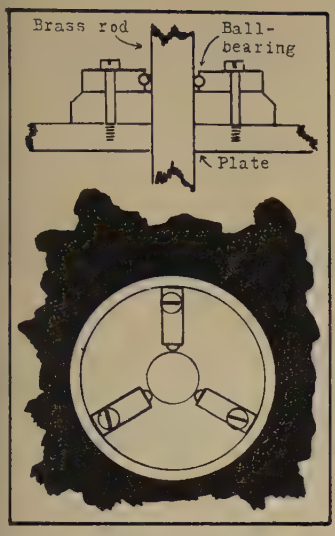


Figure 2. Side and top view of new mechanism.

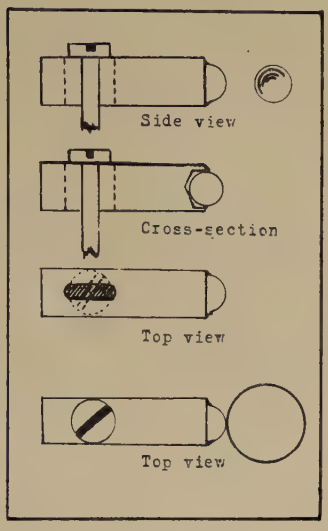


Figure 3. Views of new brass bar and bearings.

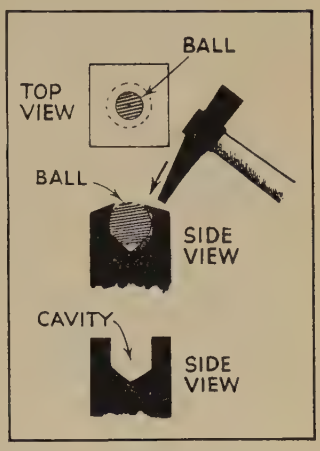


Figure 4. Method of mounting ball-bearings.

This simple mechanism has fulfilled all the requirements for which it was created in 1947. An immediate increase in the ease with which the pen moved and a greatly improved sensitivity for the weakest gusts were achieved. Finally, the variability of the zero line disappeared altogether.

Some time has passed since the anemograph began functioning in its new form and the innovation has offered no obstruction whatsoever to the proper functioning of the instrument and all the advantages have been maintained. Furthermore, the use of steel ball-bearings in a brass bar to support a moving brass rod does away completely with the problem of lubrication.

The author wishes to express his thanks to Sr. Octavio Rosales V. of the Instrument Shop of the Meteorological Office of Chile for his expert execution of the new mechanism.

551.507.321

The drag of spherical rubber balloons

By W. W. MAPLESON*
Department of Physics, Durham Colleges in the University of Durham

(Manuscript received 10 July 1953, in revised form 11 February 1954)

SUMMARY

An empirical relation $C_d = 2{\cdot}7 \times 10^5 R^{-1}$ is given between drag coefficient, C_d, and Reynolds number R, for a captive, nominally spherical, rubber balloon, for the range $\log_{10} R = 5{\cdot}1$ to $5{\cdot}8$. It is suggested that the drag increases as the shape of the balloon progressively deviates from the spherical. A calibration of a Meteorological Office radiosonde windmill as a cup anemometer is also included.

1. INTRODUCTION

In the course of an investigation into point discharge in atmospheric electricity in which a discharge point was attached to a captive balloon, it was required to know the wind speed at the height of the balloon from moment to moment. As the exact height of the balloon was also required it was convenient to observe the inclination to the vertical of the direction from the mooring post to the balloon and determine the relation between this and the wind speed.

* Now with the Department of Anaesthetics, Welsh National School of Medicine, Cardiff.

This relation was determined experimentally. The corresponding relation between drag coefficient and Reynolds number is considerably at variance with the results of wind-tunnel and dropping experiments on true spheres, Goldstein (1938), and covers a range of Reynolds number different from that in the recent work of Ludlam (1953).

2. Method

The method consisted in attaching a light cup anemometer to the flying cable, just below the balloon, and recording the wind speed (according to the anemometer) and the inclination of the balloon.

3. Calibration of anemometer

A Meteorological Office radiosonde windmill was used as the anemometer. It was calibrated by fixing it 2 m below and 40 cm across wind from the head of a Dines anemometer. The rotations, which periodically opened an electric contact, were indicated by a d.c. relay or an a.c. circuit and earphones. The wind speed, V, from 2 to 30 km/hr, could be reasonably well expressed in terms of N, the number of rotations per minute, by

$$V = (0{\cdot}125 \pm 0{\cdot}003)\, N - 0{\cdot}114 \pm 0{\cdot}017$$

where the errors represent the standard deviations. The fact that $N \neq 0$ when $V = 0$ could not be attributed to any experimental error so it was concluded that there must be a rapid fall in the rate of rotation of the windmill for wind speeds less than 2 km/hr.

4. Calibration of balloon inclination in terms of wind speed

700 g double-necked spherical rubber balloons were used and inflated to a diameter of nearly 2·1 m. The balloons were not balanced for any particular lift, the filling in every case being simply the contents of one cylinder of hydrogen : nominally 165 ft^3 but actually, according to the suppliers, about 160 ft^3, giving a gross lift of about 5·0 kg. The anemometer was attached to the flying cable (a whipcord of about 1·5 mm diameter) about 3 m below the balloon, by means of a balsa-wood framework, so that its axis of rotation was at an angle of about 15° to the flying cable. Since the inclination of the balloon never exceeded 36° in the calibration experiments the axis of the anemometer was never far from perpendicular to the wind direction. One 26 and one 28 s.w.g. copper wire connected the anemometer to the ground. A protractor with a freely swinging point was clipped on to the flying cable near the ground for the measurement of inclination, due correction being made for the fact that the flying cable did not lie on a straight line.

Three experiments were made. In the first the balloon was flown from a 50-m length of flying cable, the total load being 1·25 kg and the net lift therefore 3·75 kg. Observations of wind speed (according to the anemometer) and inclination were made for a period of two hours covering a range of inclination, θ, from 0° to 36°. In the second and third experiments a fresh balloon was used, with a 100-m length of flying cable. The total load was 1·62 kg and the net lift therefore 3·38 kg. The second experiment constituted 44 min of observation soon after the balloon was inflated, covering a range of θ from 0° to 23°, while the third experiment, about two hours later, constituted about eight minutes of observation immediately before the balloon burst, covering a range of θ from 7° to 12°.

5. Results

One-minute averages were taken of θ and V. For the immediate problem a relation was established between $\tan\theta$ and V but, to put the results into a more general form, corresponding values of the drag coefficient, C_d, and Reynolds number, R, have been calculated on the assumption that the balloon was a true sphere and that the flying cable and copper wires contributed a negligible amount to the drag. The results can then be reasonably well represented by a relation of the form

$$C_d = bR^{-1}$$

in which the values of b and ranges of R for the three experiments were respectively :

$$b_1 = (3{\cdot}09 \pm 0{\cdot}04) \times 10^5 \qquad 10^{-5}R = 2{\cdot}1 \text{ to } 7{\cdot}0$$

$$b_2 = (2{\cdot}27 \pm 0{\cdot}07) \times 10^5 \qquad 10^{-5}R = 1{\cdot}3 \text{ to } 4{\cdot}6$$

$$b_3 = (3{\cdot}01 \pm 0{\cdot}13) \times 10^5 \qquad 10^{-5}R = 1{\cdot}4 \text{ to } 2{\cdot}7$$

where the errors represent the standard deviations.

The differences between b_1 and b_2 and between b_3 and b_2 are clearly significant but that between b_1 and b_3 is not significant.

6. Discussion

The low value of b_2 was obtained for the second balloon soon after inflation, when its shape was very close to a true sphere; the high value of b_3 was obtained just before it burst, when the shape was considerably distorted; and the high value of b_1 was obtained for the first balloon which was considerably distorted when it was first inflated. Thus it seems probable that the drag of these balloons increases as their shape progressively deviates from the spherical.

The present results suggest that this phenomenon could explain the whole of the $\pm$ 15 per cent variation found by the Meteorological Office (1952) in the rate of ascent of different specimens of this type, all balanced for the same net lift. Some of the variation in the present case might, of course, be due to variation in the filling of the hydrogen cylinders – but very little since the suppliers estimate that this would not exceed $\pm$ 2½ per cent, and the resulting variation of b would be much smaller than this.

For an average balloon b would be somewhere between b_1 and b_2, say about $2{\cdot}7 \times 10^5$. Then for the two limits of the range of Reynolds number covered by the present work ($\log_{10} R = 5{\cdot}1$ and $5{\cdot}8$) the values of C_d are 2·08 and 0·41 respectively. The highest values of C_d quoted by Goldstein for spheres at these Reynolds numbers are for dropping tests, and amount to 0·51 and 0·27 respectively. The high values of drag coefficient in the present results are probably due partly to the fact that the balloon is not a true sphere and partly to the disturbances generated at the necks – particularly the lower, holding, neck which necessarily was always upstream of the great circle facing the wind.

Acknowledgments

The author's thanks are due to Dr. J. A. Chalmers for advice and encouragement, to the Meteorological Office for the radiosonde windmill, to the Durham Colleges Research Fund for grants for other equipment and to the Academic Board of King's College, Newcastle, for a British Association Studentship.

References

Goldstein, S. 1938 *Modern Developments in Fluid Mechanics*, Oxford (Clarendon Press), p. 495.

Ludlam, F. H. 1953 *Met. Mag.*, London, **82**, p. 306.

Meteorological Office 1952 Private communication.

551.521.3 : 535.34

Spectral line shape and its effect on atmospheric transmissions

By G. N. Plass

In a recent discussion of spectral line shape, Curtis and Goody (1954) state that the ' attempt to treat (this) problem . . . by Plass and Warner (1952a and b) . . . is unacceptable.' We believe not only that their criticism of our work is unjustified, but that there is little basis for their uncritical acceptance of the Lindholm theory as ' the most complete of modern theories.' Further, Plass (1952) has already proved that the reduction of laboratory data to atmospheric conditions is independent of line shape for a very general class of models which includes as a special case the statistical model assumed by Curtis and Goody.

First we discuss the three points that Curtis and Goody criticize in our work.

(1) Curtis and Goody do not accept our work for the Elsasser band because ' the asymptotic expressions . . . (at) the line centre . . . diverge.' In this approximation (variously known as ' square-root,' ' black line centre,' ' strong line ') which has been widely used and accepted by infra-red spectroscopists for several decades, the asymptotic expansion is always made in this way. Clearly, whenever there is complete absorption for several half-widths away from the line centre, any value of the absorption coefficient equal to or greater than the actual value can be assumed for this interval. Then the calculated absorption correctly gives total absorption over this frequency interval, e.g. Elsasser (1942). The crucial point in any derivation of a transmission function is to use the correct absorption coefficient for the range of frequencies for which there is neither complete absorption nor complete transmission; this was done in a consistent manner in our calculations. This is discussed in Plass and Warner (1952b), Section 5.

(2) Curtis and Goody state that our line shape ' is not normalized.' Nevertheless our line shape is correctly normalized to terms of the first order in $M\alpha$. In fact the Lindholm line shape which we used (Plass and Warner 1952b, Eqs. (1), (2), (3)) is identical in all important particulars with the one finally adopted by Curtis and Goody, Eq. (18). The only differences are : (i) The factor $(1 + 0{\cdot}193\,\epsilon)$ in the denominator. This normalizes the line shape to terms of the next order in $M\alpha$. For the values of the parameters given by Curtis and Goody in Table 1, this correction to the result is never more than 1·8 per cent. (ii) The extra term ' $-0{\cdot}286\,\mu$ ' in the centre of the line and the additional term $|\mu|^{-7/3}$ in the red wing. These are higher order terms which we did not consider, since the Lindholm line shape certainly is not accurate to this approximation.

(3) Curtis and Goody believe that the ' single-line model is misleading.' This is not the case when it is used within its range of validity. For example, Plass (1954) has shown that the single-line equation is valid in the atmosphere when $\gamma < (2\pi\alpha/d)^2$, where $\gamma = Sc\rho_s/2\pi\alpha_s g$ and d is the spacing between lines. This derivation assumes an isothermal atmosphere and the Lorentz line shape, but would not be appreciably different for non-isothermal conditions and an asymmetric line shape. At one-atmosphere pressure, the single-line model has a somewhat limited range of application, but the range of validity increases rapidly as the pressure decreases, since it is proportional to the square of the pressure.

Whether or not the single-line equations can be misleading depends on the method that is used to calculate atmospheric transmission functions. One possibility is to make an entirely theoretical calculation. A large programme to compute the relevant quantities with the aid of an electronic computer has been started in this country by Kaplan. Whether or not these calculations are ' very suspect ' depends on the care with which they are made. The article by Plass and Warner (1952b) refers entirely to this approach. They show that the line shape in the wings can make a considerable difference in the results from such calculations for the 15 μ CO_2 band, for example, where neither the statistical nor Elsasser model is a good approximation.

The other approach is to use ' theory . . . only in order to apply existing laboratory data to atmospheric conditions.' Curtis and Goody show that the exact line shape is unimportant for the statistical model. Plass (1952) has already shown that a simple method can be used to reduce the laboratory data to atmospheric conditions. This method is valid for any line shape that has yet been proposed and does not make any assumptions regarding the distribution of intensities and the spacing of the spectral lines in the band. In particular it is valid for the statistical model itself. This method is valid only for path lengths greater than or less than

certain critical amounts which can be determined experimentally, but this restriction has never proved bothersome to us in practice.

With regard to the possible influence of a shift of the line centre, Plass and Warner (1952b) showed that this caused only a very small change in the transmission function from the top of the atmosphere down to a given level for an isothermal atmosphere and a constant mixing ratio. Later work of ours, which we did not believe was of sufficient general interest to publish, shows that it is also a very small correction even for the transfer of radiation between any two arbitrary levels.

A much more important point than the above details is that the Lindholm line shape almost certainly does not apply to infra-red molecular absorption spectra. Our articles (1952) were written to call the attention of meteorologists to some of the unsettled problems in connection with line shape. In particular we clearly pointed out that the basic assumptions of the Lindholm theory might not be reasonable for our problem and that this shape was used in most of our work only because it is the most easily integrated, asymmetrical line shape and thus might indicate whether deviations from the Lorentz shape could be of meteorological importance.

This opinion of the inapplicability of the Lindholm theory has been greatly strengthened by further studies since we wrote our original papers. The Lindholm theory does describe the line shape for *atomic* spectra in the visible and near infra-red. However, since there are frequency perturbations with both positive and negative signs for molecules (unlike the atomic case where they are all of one sign), *the Lindholm theory is invalid for the molecular infra-red spectra that are of interest to meteorologists.*

The only complete modern theory of the shape of *molecular* spectral lines is that of Anderson 1949). This paper together with those of Margenau and Watson (1936), Spitzer (1940), Foley 1946), and Holstein (1950) presents a complete picture of the factors that determine the line shape in the infra-red. Recent experiments of Benedict and Silverman (1954) show that the Lindholm line shape cannot be valid for molecular lines in the infra-red. They show further that the lines are not appreciably asymmetrical even at many half-widths from the line centre, although the Lorentz line shape does *not* hold far from the line centre. This is consistent with the above theories. Benedict and Silverman plan to discuss both the experimental and theoretical results concerning line shape in a forthcoming article.

Although Anderson (1949) has given the general theory of infra-red molecular line shape, unfortunately at present it does not appear possible to calculate the line shape many half widths from the line centre, since such frequencies are emitted by molecules making very close collisions. For distant collisions the interaction of the molecules may be taken to be the r^{-6} dependence of the van der Waals forces, for the cases of meteorological interest. However, there are r^{-8} and other higher order terms in this expansion that become increasingly important for close collisions. For a very close collision the force becomes repulsive and varies extremely rapidly with distance. Thus the intermolecular forces between the colliding molecules in the states corresponding to the transition in question must be known even at very small distances. Such information is available at present for only a very few simple molecules, and then only for the ground state and the first few excited states. Thus it would seem that the experimental determination of the line shape is the only method that can resolve this question in the near future. These fundamental questions of line shape are of primary interest to physicists, but they are of more than academic interest to meteorologists, since the line shape does introduce an unknown error in all calculated radiation fluxes that are based entirely on theoretical determinations of the band constants.

On the other hand, if laboratory data are used to determine the atmospheric radiation flux, the question of the exact line shape in the far wings is unimportant. We do agree with Curtis and Goody, but for different reasons, that it is more important first to correct other types of errors in radiation calculations. It is most important to have measurements of the absorption of all the relevant infra-red bands of the scope and accuracy of those made for nitrous oxide by Goody and Wormell (1951). It still seems to us that ' if the absorption lines in the lower atmosphere had a different shape from that which they have, then the weather would not be what it is ' (Goody 1952).

References

Anderson, P. W.	1949	*Phys. Rev.*, **76**, p. 647.
Benedict, W. S. and Silverman, S.	1954	*Bull. Amer. Phys. Soc.*, **29**, p. 15.
Curtis, A. R. and Goody, R. M.	1954	*Quart. J. R. Met. Soc.*, **80**, p. 58.
Elsasser, W. M.	1942	*Harvard Met. Studies*, No. 6.

Foley, H. M.	1946	*Phys. Rev.*, **69**, p. 616.
Goody, R. M.	1952	*Quart. J. R. Met. Soc.*, **78**, p. 640.
Goody, R. M. and Wormell, T. W.	1951	*Proc. Roy. Soc.* A, **209**, p. 178.
Holstein, T.	1950	*Phys. Rev.*, **79**, p. 744.
Margenau, H. and Watson, W. W.	1936	*Rev. Mod. Phys.*, **8**, p. 22.
Plass, G. N.	1952	*J. Opt. Soc. Amer.*, **42**, p. 677.
	1954	*J. Met.*, **11**, p. 163.
Plass, G. N. and Warner, D.	1952a	*Phys. Rev.*, **86**, p. 138.
	1952b	*J. Met.*, **9**, p. 533.
Spitzer, L.	1940	*Phys. Rev.*, **58**, p. 348.

Department of Physics,
The Johns Hopkins University,
Baltimore 18, Maryland, U.S.A.
25 March 1954.

Reply

By A. R. Curtis and R. M. Goody

Dr. Plass's letter does not put any clear issues before us. He correctly remarks that radiation problems can be solved either by working directly from the various molecular constants or indirectly from laboratory data, and that we are concerned with the latter approach. Our paper was a tedious but necessary step as far as this method is concerned. It is correct, as far as it goes and according to its assumptions, or at least Dr. Plass points to no errors. It uses methods and assumptions similar to those used in Plass and Warner's papers, and where these papers can be construed as treating the problem which we define they are certainly unacceptable. Dr. Plass gives no numerical demonstration to show that the approximations which we name are not responsible for the great differences between our two results, so that there is no specific point here for us to deal with. However since it has been suggested that our reluctance to accept Plass and Warner's work is unjustified we will briefly amplify our reasons by pointing out that :

(i) the use of line wing approximations alone requires black-out for $|\nu - \nu_0| \leqslant 2\nu_p$ (*not* several half-widths from the line centre; see our Fig. 1) which, on our data, limits the region of validity to total blackout for most atmospheric bands;

(ii) the approximations made to the Lindholm shape (some of which are not of ' higher order ' according to any definition known to us) give an answer which is more characterisic of the approximations than of the already inadequate physical assumption of van der Waals interactions;

(iii) only a very small part, near the origin, of the ' single line ' diagram in Plass and Warner (1952a) is in the range of validity for our problem.

This difference between us forms the main contentious point, although a number of other matters are raised.

Having recognized the distinction between an *a priori* theoretical method and a semi-empirical method, Dr. Plass should find no difficulty in construing the apparent paradox shown by his final quotation. In fact he gives the answer elsewhere in his letter and it will also be found in our introduction, namely, that the line shape may influence the far wings of a band but nevertheless laboratory spectra can be used without knowing exactly what it is.

Dr. Plass has great confidence in his treatment of the problem by means of separate solutions for the ' strong ' and ' weak line ' approximations. Our experience has clearly been less fortunate than his, since we find that too many cases of interest fall outside the range of validity of either, and therefore we require a less restricted approach. He is completely mistaken when he states that a treatment in terms of asymptotic forms includes all models as special cases, since one of the main uses of a model is to avoid recourse to these approximations.

A useful piece of information in this letter concerns the experimental work of Benedict and Silverman. When it is published it will be interesting to see if our conclusions need modification. Since enough saving clauses are given at the beginning of our Section 3 to guard against a development of this kind, we must give grudging admiration to Dr. Plass's ingenious attempt

to differentiate between his and our uses of Lindholm's work by castigating ours as uncritical. Lindholm's work was used by us since it is the only treatment which is sufficiently explicit, and which avoids the fundamental errors in the Lorentz theory; it seems that we differ mainly in our desire to avoid mutilating the line centre.

Department of Mathematics,
Sheffield University.
25 May 1954.

Department of Meteorology,
Imperial College.

551.574.1 : 536.42

The freezing of cloud droplets

By S. C. Mossop

In a recent paper E. K. Bigg (1953a) has reported observations of the freezing points of droplets of distilled water ranging in diameter from 0·1 mm to 2 cm. He has then applied these results and some later measurements on smaller drops to the freezing of cloud and raindrops (Bigg 1953b). He comes to the important conclusion that freezing nuclei may be unimportant in glaciation in clouds below a temperature of — 20°C; their effect will be masked by the spontaneous freezing of the cloud droplets. Where large drops are present, as in cumulus, this limit may be as high as — 13°C.

From the fact that distilled water from various sources gave the same mean freezing temperatures, it was concluded that Bigg's curve (shown as the full line '1953b' in Fig. 1; this is an amendment of the earlier curve '1953a' as shown pecked) represents the diameter-freezing point relationship for spontaneous nucleation of water. It was supposed that the impurities present were without effect upon freezing.

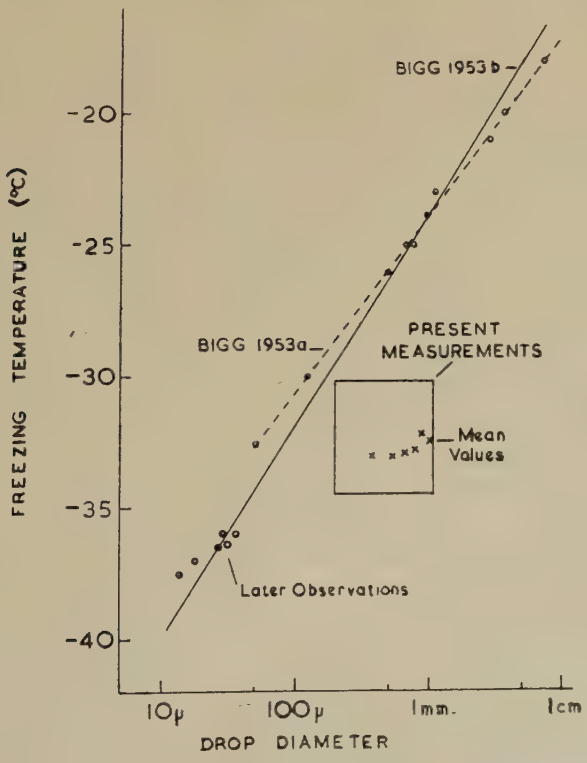


Figure 1. The freezing temperature of water drops as a function of their diameter.

That this assumption was not justified is shown by measurements by the present author of the freezing points of samples of water contained in pyrex capillary tubes. These samples were made by the condensation of steam directly into freshly-drawn capillaries. Provided that all room air was first driven out of the steam generator, and the capillaries were then drawn from wider tubing through which a stream of ' clean ' steam was passing, specimens of water could be prepared whose freezing points were consistently lower than the corresponding values on Bigg's curve for the same droplet volume. Seventy-one specimens ranging in volume from $0{\cdot}05 \times 10^{-3}$ to $6{\cdot}4 \times 10^{-3}$ cm^3, and in diameter from 0·15 to 0·52 mm, gave a mean freezing point of $-$ 32·9°C with a standard deviation of 1·3°C. The cooling rate was 0·5°C/min. On plotting freezing points against ' equivalent diameter,' i.e., the diameter of a spherical droplet of equal volume, it was found that all the points fell within the rectangle shown in Fig. 1. The mean freezing points for various size ranges are also plotted.

The fact that it is possible, by careful exclusion of room-air impurities, to prepare water samples of freezing points consistently lower than Bigg's curve shows that this does not represent the diameter-freezing point relation for spontaneous nucleation. It appears that the freezing points of drops represented by the pecked line, ' 1953a,' were affected by the presence of impurities. His later measurements on small drops and the present observations may possibly represent cases of spontaneous nucleation; but this can only be tested by further experiment.

Thus it does not appear to be valid to apply Bigg's results to glaciation in clouds on the assumption that they represent spontaneous freezing phenomena.

A fuller description of the experiments mentioned above will be published later.

References

Bigg, E. K. 1953a *Proc. Phys. Soc.* B, **66**, p. 688.
1953b *Quart. J. R. Met. Soc.*, **79**, p. 510.

The Clarendon Laboratory,
Oxford.
27 May 1954.

Reply

By E. K. Bigg

Mossop implies that by obtaining a mean freezing temperature of a given volume of water lower than that found in my experiments, my contention that impurities in the water played a minor role in nucleation is disproved.

I consider that this standpoint ignores a fundamental difficulty inherent in experimental studies of spontaneous nucleation. The theory of spontaneous nucleation deals with nucleus formation in a homogeneous medium uninfluenced by boundaries. In practice one must use a volume surrounded by some material other than water, which may or may not influence freezing. If experiments are required to give information on the freezing of spherical airborne drops, then they are best performed on such drops or at least, as in my work, on the supposedly analogous case of spherical drops in a liquid. The use of capillary tubes, or indeed any solid container, or support, cannot be assumed to give results relevant to the glaciation problem in clouds. In fact, it has often been shown that a given volume of water can be supercooled further in a narrow capillary than in a wide tube, and that solid surfaces have a considerable and individual effect on freezing when in contact with the cooled liquid.

To illustrate this, I would like to call attention to a paper by Dr. C. L. Hosler on ' Factors governing the temperature of ice-crystal formation in clouds,' read at the Toronto Meteorological Conference, 1953 (in press). Here, experiments were described covering the influence of volume, cross-sectional area and radius of capillary tubes on the freezing of water contained in them, which led to the conclusion that the radius of the tube was the vital parameter in determining the freezing temperature. The interpretation placed on this result was that the molecular re-adjustment necessary in the water at the water-glass interface reduced the probability of freezing. Whether or not Hosler's theory is correct, his work shows that Mossop's results should not be applied, without careful examination, to problems other than the freezing of water in pyrex capillaries.

Thus I would conclude, not that Mossop's results conflict with mine, but rather that we have measured fundamentally different quantities.

Division of Radiophysics,
C.S.I.R.O., University Grounds,
Sydney.
15 June 1954.

551.509.34 : 551.524.3 : 551.574.4

Night cooling under clear skies

By J. L. Monteith

In a letter published in the January issue of the *Quarterly Journal* (p. 105), Mr. W. E. Saunders quotes a paper by Seeman and Löw (1944) as evidence that dew formation is responsible for the discontinuities in nocturnal cooling which he has recently been studying (Saunders 1952). I should like to present four reasons for rejecting this 'dew' hypothesis and returning to an alternative explanation (Saunders 1949) for the apparent correlation between the rates of condensation and heating which Seeman and Löw discuss.

Firstly, it has been implicitly assumed that the formation of dew *suddenly* introduces a positive term of significant magnitude into the heat-balance equation of the surface. In fact, the onset of condensation is a gradual process. All natural grass surfaces contain leaves of different sizes and it is common to find patches of clover, plantain, etc., which have larger leaves than grass. The larger the leaf, the less favourable is the heat exchange and the lower is the temperature. I have observed that dew forms consistently on clover and plantain half an hour to an hour before grass becomes moist. Dew formation would occur suddenly only over an extremely homogeneous surface such as is unlikely to be found in practice and measurements made in a playing-field with a dew-balance (Jennings and Monteith 1954) suggest that the rate of latent-heat release in the first half hour of condensation is at least two orders of magnitude less than the net long-wave radiation flux for clear skies.

Secondly, the observations of Seeman and Löw and of many other workers have shown that air temperatures vary irregularly during calm 'radiation' nights. Temperature increases occur *without destruction of the inversion*, however, which implies that the mechanism of heat transfer is either turbulent diffusion from *upper* levels or else a sudden increase in the outgoing radiation flux produced by a rise of surface temperature (as at sunrise). If such a temperature increase *were* initiated by the release of latent heat, it would immediately be self-quenched since the surface temperature would be raised above the dew point and dew formation would cease. Large increases in the temperature of surface air (Seeman and Löw report a case where the rise at 10 cm was 3°C) cannot therefore be produced by dew formation.

Thirdly, from the Rye observations of Best *et al.* (1952) it appears that the discontinuity occurred at heights up to 100 m but there is no evidence of the time lag that would be expected if the origin of the discontinuity were at the ground. Furthermore, the temperature discontinuity was often clearly associated with a sudden increase in humidity at all levels and for the mean of six occasions in March the two discontinuities occurred almost simultaneously five hours after the humidity inversion had reached a height of 15 m, i.e., at least five hours after the formation of dew (Pedlow 1952).

Fourthly, on a number of occasions in the autumn of 1953, I have observed the temperature discontinuity, using a thermograph with the element about 13 cm above the ground in a large open playing-field. The times when the first traces of dew appeared were noted and an analysis of 10 nights on which dew formed shows :

Number of nights with no temperature discontinuity	4
Number of nights when dew formed more than an hour before discontinuity	4
Number of nights when dew formed less than an hour before discontinuity	2

Supported by this body of evidence, dew must surely be acquitted on the charge of producing temperature discontinuities. There is a strong case, however, for attributing this type of discontinuity to a sudden burst of turbulent activity which might be expected to occur after sunset

when the wind shear reached some critical value, and to recur periodically thereafter. No satisfying explanation exists in terms either of a Richardson number or of any other relation between the flow parameters but the effect is well known and well documented.

A breakdown of this kind would lead to an immediate increase in the downward transport of momentum and of heat and water vapour below the appropriate inversion levels as observed at Rye and elsewhere. The rate of condensation on a dew-plate such as that used by Seeman and Löw would increase also as the result of increased mixing at the ground. On this basis, it is unnecessary to postulate a dubious cause-and-effect relation between condensation rate and cooling rate changes : both are produced by a third process.

References

Best, A. C., Knighting, E., Pedlow, R. H. and Stormonth, K.	1952	*Geophys. Mem. Met. Office*, London, No. 89.
Jennings, E. G. and Monteith, J. L.	1954	*Quart. J. R. Met. Soc.*, **80**, p. 222.
Pedlow, R. H.	1952	*Air Ministry, Met. Res. Ctee., M.R.P.*, No. 742.
Saunders, W. E.	1949	*Quart. J. R. Met. Soc.*, **75**, p. 154.
	1952	*Ibid.*, **78**, p. 603.
Seeman, J. and Löw, K.	1944	*Met. Z.*, **61**, p. 158.

Imperial College,
London.
19 May 1954.

Reply

By W. E. Saunders

It is considered that Mr. Monteith's results with his dew-balance (Jennings and Monteith 1954) dispose of the possibility that latent-heat release by dew condensed *from the air near the ground* can be the cause of the temperature discontinuity. The results show there is only a very gradual increase in weight throughout the night, and this is confirmed by Mr. Hirst, examining a single plant (Hirst 1954).

However it is apparent that no dew-balance which can only take account of an increase in weight of a sample of the surface, or of an individual plant, gives the whole picture of what occurs on the grass, since no record of water produced by the vegetation can be included.

I am a little reluctant to return without further consideration to the breakdown in laminar flow as the cause, attractive as that theory appears in some ways, because there are certain important aspects not covered.

Firstly, if we accept breakdown in laminar flow, it is not clear why, when we come to regression equations for the screen-level temperature of discontinuity, we have this close relation with dew point as well as with temperature. At Northolt (Saunders 1952) and at Exeter (Saunders 1954) we find expressions which give the temperature of screen-level discontinuity (T_r) in terms of maximum dry bulb and dew point with standard deviations around 1°F. At Shawbury, on a lighter soil, Parry (1953) had the quite remarkable result of a regression equation deduced from 131 cases which gave T_r with standard deviation as small as 0·23°F. This close relation with dew point cannot possibly be coincidental, and, since the value of the afternoon dew point is in part determined by evaporation from the underlying surface, it seems strongly to imply a connection between the temperature discontinuity and the underlying surface.

Secondly, there is the comparative time regularity. It is difficult to see why a breakdown in laminar flow should behave at all consistently with regard to time, whereas screen-level discontinuities mostly occur within half an hour of the mean time, even using the fairly crude method of interpolating from half-hourly or hourly temperature readings, except when the soil condition is changeable. The level at which we do apparently have a change occurring with great time regularity is the grass level. On a clear calm evening we have slight moisture on the grass, possibly from before sunset, and at about dusk there is a sudden copious appearance of moisture. This is not shown on the dew balances, and is therefore derived from the underlying surface, not from the lower atmosphere. At this time saturation is reached on dry- and

wet-bulb thermometers supported just above the grass. The times of this occurrence show little change from night to night over a well-kept lawn (no clover, no plantain !), and the observed times (GMT) at Ickenham in June 1950 were :

On the 9th, 2010; 10th, 2007; 15th, 2009; 16th, 2000; 23rd, 2005.

Jennings and Monteith (1954) recognize the existence of this additional water from records on filter paper. They attribute it either to distillation from warmer soil to cooler grass, or to condensation of water transpired from grass during the night. As regards distillation, there is no apparent reason to expect any time regularity. As regards transpiration, the suggestion seems out of accord with the contribution made by Dr. Penman to the discussion on 18 February 1953 (*Q.J.*, **79**, p. 438), when the *termination* of transpiration was suggested as the cause of the discontinuity. The question how this water is suddenly deposited is a matter for the biologist, but its presence may readily be observed, and it may affect the temperature discontinuity in two ways : (i) by release of latent heat, (ii) by increasing the conductivity of the underlying surface. Before finally accepting laminar flow as the explanation, I think we should get to know more about what occurs on the grass, and a contribution from a biologist on this point would be most useful.

Turning to the four points raised by Mr. Monteith, the first and fourth are involved in this question of what we are to regard as ' dew.' With regard to his second point, I think there is self-quenching, and that this is the cause of the numerous further discontinuities which follow the original discontinuity. His third point, based on the Rye observations, is certainly suggestive, but, I would suggest, not conclusive in itself, since the observations were at 20-min intervals, not continuous records.

This letter is published with the permission of the Director of the Meteorological Office.

References

Jennings, E. G. and Monteith, J. L.	1954	*Quart. J. R. Met. Soc.*, **80**, p. 222.
Hirst, J. M.	1954	*Ibid.*, **80**, p. 227.
Saunders, W. E.	1952	*Ibid.*, **78**, p. 603.
	1954	*Met. Mag.*, London, **83**, p. 9.
Parry, T. H.	1953	*Ibid.*, **82**, p. 368.

Meteorological Office,
H.Q. No. 19 Group, Royal Air Force,
Mount Batten, Plymouth.
3 June 1954.

551.513 : 551.515 : 551.557 : 551.524.7

A comparison of hemispheric circulations with particular reference to the western Pacific

By W. J. GIBBS

(Read 17 February 1954. Presented by Mr. J. M. Craddock. See *Q.J.*, **79**, p. 121)

MR. E. GOLD : I do not think the westerly average component would have a value as great as that mentioned if it were to apply to the complete belt across Asia as well as America and the Atlantic. But I believe Mr. Gibbs was referring primarily to this latter section.

DR. R. C. SUTCLIFFE : We shall do well to keep in mind the limited longitudinal belt over which the upper-air comparisons are made, as in the northern hemisphere there are very large variations with longitude and the region chosen is a very special one.

The occurrence of many anticyclonic centres in the middle latitudes of the northern hemisphere, in contrast to the southern hemisphere, whether it is a result of orography or of thermal effects, has certainly a geographical cause.

A likely explanation lies in the tendency for the establishment of very long ' climatological ' waves in the northern hemisphere such as are the subject of Dr. Smagorinsky's mathematical paper to be discussed later this evening. These waves are maintained by the boundary conditions and tend to be stationary and dynamically stable. From time to time however they become unstable and then throw up large-amplitude long waves with both cyclonic and anticyclonic cells, the wavelength being imposed on the system. In the southern hemisphere, by contrast, there is little tendency for a wavelength to be imposed and the atmosphere freely selects shorter wavelengths of maximum instability which have smaller latitudinal amplitude and produce a succession of baroclinic depressions and ridges, rarely anticyclonic centres in the westerlies.

Mr. Gibbs's paper will be useful not only as factual information but also in presenting problems for the theoretician.

MR. L. C. W. BONACINA : I think the paper well illustrates the great complexity of the northern-hemisphere westerly circulation which is constantly being obstructed and modified by its continental masses. In the southern hemisphere the subtropical anticyclones are known to move fairly regularly round the globe from west to east, and perhaps this throws light on the statement of the author that the high-pressure centres on the charts of mean pressure are ' mythical.'

MR. J. M. CRADDOCK (*in reply*) : I agree with Dr. Sutcliffe and Mr. Bonacina that whereas the sector discussed may be representative of the southern hemisphere it is rather an exceptional part of the northern hemisphere, a fact to be remembered in considering the various comparisons made.

I would also like to ask Mr. Gibbs to give us some more information about the analysis in the southern hemisphere. I am sure the charts used were the best available, but I wonder whether the observational network in the southern hemisphere is good enough to determine the positions, depths and movements of at any rate the major pressure systems, without the analysis depending very much on the technique followed by the individual analyst.

MR. W. J. GIBBS (*in reply, communicated*) : Mr. Craddock's doubts concerning the adequacy of the network in the southern hemisphere have some justification. The isolated reports from ocean areas contribute to differences in interpretation, particularly in frontal analysis. However, the major pressure systems are of such area and intensity as to enable conclusions regarding their geographical distribution to be drawn with confidence that the general pattern is accurate.

I am less confident of the mythical character of the semi-permanent high-pressure cells of the Indian and South Pacific Oceans, to which Mr. Bonacina has referred. Networks in these regions are very poor. However, at least in the Indian Ocean, reports from Amsterdam Island, and from ships in the vicinity, indicate that depressions occur in the area of the ' semi-permanent anticyclone.'

As mentioned by Mr. Craddock, Mr. Gold and Dr. Sutcliffe, the limited longitudinal extent of the upper-air data would seem to prevent any general comparisons of the upper-air circulations of the two hemispheres. However, the symmetrical distribution, in the southern hemisphere, of the surface pressure systems encourages the belief that upper-air conditions in the Australian

sector are reasonably representative of the mean upper-air circulation of the hemisphere. The northern-hemisphere mean upper-air circulation is known to vary considerably with changes in longitude. Nevertheless, the zonal wind pattern for the western North Pacific shown in my paper shows the same features as the mean cross section for the hemisphere presented by Petterssen at the centenary proceedings of the Society.

For winter Petterssen shows a west wind maximum of about 80 kt at approximately 200 mb and 25°N; my cross section has a maximum of 110 kt at about 200 mb and 30°N. For summer Petterssen indicates a maximum of 30 kt at 200 mb and 42°N; I have shown a 40 kt maximum at 200 mb and 37°N. Undoubtedly in winter the jet stream is stronger in the western North Pacific than in the rest of the hemisphere, but this sector seems reasonably representative of the mean circulation of the hemisphere, and certainly much more representative than the North American sector.

My intention was to show that, although there are large longitudinal variations in the northern-hemisphere circulation, and although there are basic differences between the surface circulations of the northern and southern hemispheres, the mean upper-air circulations of the two hemispheres are generally the same.

551.513.1

The dynamical influence of large-scale heat sources and sinks on the quasi-stationary mean motions of the atmosphere

By J. SMAGORINSKY

(Read 17 February 1954. See *Q.J.*, **79**, p. 342)

MR. J. S. SAWYER : I wish to ask Dr. Smagorinsky how far he considers that the effects which he attributes to surface friction are dependent upon the somewhat arbitrary way in which friction has been introduced into the equations.

DR. A. W. BREWER : An approach to meteorological problems which has a thermodynamic basis can only be expected to give reasonable results if friction is present, thereby requiring that mechanical energy shall be produced. To produce this energy we require a heat source at high pressure and a heat sink at low pressure. If we change locally either the source or the sink we shall expect the greatest effects, but if we add or remove heat at the middle of the atmosphere then the effect should be small, rather like standing on a see-saw at the centre.

As regards the subsidence which Dr. Smagorinsky finds over a thermal sink in the frictional case, we have observed most exceptionally dry air (0·6 per cent RH at 5,000 ft) in what appeared to be an outflow from the Siberian high in February. I have long thought that in the centre of the high the air must have subsided through very great depths.

DR. R. C. SUTCLIFFE : It is most satisfactory to see quantitative dynamical methods being applied to these very difficult problems of the quasi-steady states of the general circulation and to see how heating and cooling in the westerlies may set up long waves.

I think this paper is another lesson on the extreme difficulty of practical meteorological problems. The author hopes to throw light on the problems of long-range forecasting and I am sure he does so in a fundamental way. But we are delighted when he manages to show that something a little bit like the real atmosphere can be inferred from the physics. To go beyond this and get a first quantitative approximation to seasonal variability and then to seasonal anomalies, a small effect, and then further to prediction, is to meet difficulties of greater order at each stage. Unless we are very lucky in striking an unexpectedly useful result there is a long long way to travel from this paper to forecasting based on the same principles. But it is always necessary to begin.

DR. J. SMAGORINSKY (*in reply*) : Regarding Dr. Brewer's remarks I should like to reiterate the points I made in the paper regarding the energy balance in this model. With a periodic distribution of relative heat sources and sinks there is no net heat-energy contribution. Since surface friction acts only on the small perturbations, then this second-order energy must come from the basic current which is steady to the first order of small quantities.

The calculations showed the opposite of what Dr. Brewer expected to find. In the non-frictional case the heat source and sink both are at zero perturbation-pressure amplitude. When skin friction is introduced, the sink is located at *higher* pressure than is the source.

A comparison of the results of the advective model and of the general non-frictional model shows that the large-scale vertical motions are irrelevant in maintaining the balance against the non-adiabatic heating. Thus the sense of the very small vertical motions which do occur is not very significant. They may be either upward or downward over a source, the direction in the lower levels essentially being determined by the relation of the stationary wavelength to that of the perturbation heating. That skin friction generates vertical motions is fairly well established and understood and must account for the indirect observational evidence – for example, Dr. Brewer's remarks on pronounced subsidence found at the centre of the Siberian high. This brings us to Mr. Sawyer's question. I would say that the mode of incorporating the frictional influence is crude rather than arbitrary. However, I think that the qualitative and quantitative (at least in order of magnitude) influence of skin friction would be changed little by a more precise analysis.

551.515.127 : 551.547 : 551.509.317

Some downstream effects associated with large-amplitude troughs in upper flow patterns

By C. V. SMITH and A. G. FORSDYKE

(Read 17 February 1954. See *Q.J.*, **79**, p. 414)

Mr. C. V. Smith : I should like to comment on a recent suggestion that in forecasting by numerical methods, simple mathematical models might be employed to foreshadow the broad-scale evolutions for three to four days ahead. The note shows that purely barotropic evolutions are uncommon over such a period and implies that to produce extended forecasts by computing methods it is necessary, on the great majority of occasions, to proceed with mathematical models which accommodate baroclinic developments and non-adiabatic processes.

Dr. R. S. Scorer : It is interesting that although it is agreed that the development will affect the behaviour of weather systems more and more as time goes on yet, when averaging processes are used by Namias over periods spanning several systems, the barotropic model again seems to provide a fair guide.

Dr. J. Smagorsinky : In reply to Dr. Scorer, I understand that Namias does use barotropic constant-vorticity trajectories as one of the indications in formulating his 5- and 30-day forecasts. However, he *does not* assume that 5- and 30-day mean motions are governed entirely barotropically. To quote Namias, ' a trend map is constructed which is a time-smoothed estimate supposedly approximating subsequent non-barotropic influences. Vorticity trajectories are then used by a step-wise procedure to indicate barotropic unbalance and to adjust the flow depicted by the trend map ' (see for example, Namias, J., 1953, ' Thirty-day forecasting : a review of a ten-year experiment,' *Meteorological Monographs*, **2**, No. 6, Chaps. IV-VI).

551.510.41

The origin of atmospheric nitrous oxide

By R. M. GOODY and C. D. WALSHAW

(Read 17 March 1954. See *Q.J.*, **79**, p. 496)

Mr. B. J. Mason : The authors have sought to show that bacterial reactions *in the soil* can supply nitrous oxide at a rate sufficient to compensate for its decomposition by photo-chemical processes and that its formation by *homogeneous* chemical reactions in the atmosphere is much

less important. I should like to make two suggestions in this connection :

(1) It may well be that the oceans are a larger source of N_2O than the soil.

(2) Although the chemical reactions in the homogeneous gaseous phase are bound to be very slow, is it not conceivable that they might proceed very much more rapidly in the lower troposphere on the surfaces of solid particles, possibly with adsorbed water vapour acting as a catalyst ?

Mr. B. C. V. Oddie : In his opening remarks, Dr. Goody said that Bates and Witherspoon had argued that, if the soil were the main source of nitrous oxide, almost the whole of the nitrogen transferred from the soil to the atmosphere must be in the form of nitrous oxide; and this they thought unlikely. Is not this objection still valid ? We have not been told their reasons, which are presumably chemical; but could Dr. Goody answer them ?

Dr. P. W. Arnold : Firstly, I might re-affirm that the idea that soils release nitrous oxide is certainly not new. We have confirmed that moist soils containing simple compounds of nitrogen are able to release nitrous oxide both in the laboratory and in the field. In the nitrogen cycle in nature there has been a tendency to assume that as molecular nitrogen is ' fixed ' biologically, so is molecular nitrogen returned to the atmosphere by ' denitrifying ' processes. This fact alone has tended to deter investigators from examining denitrifying processes, particularly in systems which are near natural conditions, because of the analytical difficulties concerned in dealing with the minute amounts of molecular nitrogen. Estimates of the size of the nitrogen cycle have varied widely and have invariably been based on most uncertain ' fixation ' rates. Now, at least it is possible to say that nitrous oxide is probably an important end-product to the biological part of the nitrogen cycle; that a proportion, if not all, the nitrous oxide is evolved from soils; that, with certain reservations, a lower limit of 10^{11} mol cm^{-2} sec^{-1} can be put to the size of the nitrogen cycle. As has just been suggested, it would be valuable to establish whether or not the oceans contribute any nitrous oxide to the cycle. All I can say here is that the oceans are a potentially greater source of simple nitrogeneous compounds than are the soils of the world, but of course there is a good deal more to it than that.

Dr. Jane Meiklejohn : The most probable mechanism for the production of nitrous oxide in soils is a microbiological one, the reduction of nitrates by bacteria. Until recently it was thought that the main, or even the only, gaseous product of this reduction was nitrogen; however, Verhoeven, working in Holland, showed two years ago that certain spore-forming bacteria (*Bacillus* spp.) formed nitrous oxide, as well as nitrogen, by the reduction of nitrate. Bacteria of this type are quite common in soil.

Dr. T. W. Wormell : One of the by-products of the work described in this paper is an estimate of the total amount of nitrous oxide in the atmosphere; this turns out to be nearly the same as the total amount of ozone. This remarkable fact led some of us, at an earlier stage, to hazard the guess that most of the nitrous oxide was in the upper atmosphere and its mode of formation probably a photo-chemical one. The work done by Dr. Goody and Mr. Walshaw has shown that, on the contrary, most of the nitrous oxide is nearly uniformly distributed through the lower atmosphere; this means that it is of no importance in radiation problems in the stratosphere or troposphere.

Is it simply a coincidence that the total quantities of ozone, nitrous oxide and indeed of methane, found in the atmosphere, appear to be of the same order of magnitude ?

Mr. L. C. W. Bonacina : I have always understood that nitrous oxide is a product of lightning so that thunderstorm rains are regarded as rich fertilizers. As thunderstorms are always in progress somewhere or other and the concentration of nitrogen products is probably relatively high this source of atmospheric nitrous oxide should be far from negligible.

Dr. R. M. Goody and Mr. C. D. Walshaw (*in reply*) : Mr. Mason's first suggestion is answered by Dr. Arnold's statement, to which we have nothing to add. The second is relevant and could be important. Most of the ozone which reaches the lower stratosphere and the troposphere must be destroyed by catalysis, but we know so little about the subject that we were unable to make any assessment of its importance in our paper.

We prefer to refer Mr. Oddie to Bates and Witherspoon's paper. They are not very specific about the reason for their objection, and we only mentioned the point because their opinion on such matters must be given considerable weight.

To Dr. Wormell's list of minor gases of similar total amounts we may add carbon monoxide. Ozone is formed photo-chemically. Nitrous oxide is discussed in our paper and we find some support for Adel's bacterial mechanism. Methane and carbon monoxide are discussed by Bates and Witherspoon. The presence of the former is attributed to seepage from fuel beds and

anaerobic decay of vegetable matter, while the presence of the latter is attributed to combustion processes. To all appearances any similarity between these four gases is pure coincidence.

In reply to Mr. Bonacina, the idea that electrical discharges in thunderstorms have significant effect on the nitrogen cycle has now been generally abandoned. In any case there is no evidence that nitrous oxide is an important product from discharges in air.

551.558.1 : 59

The nature of convection as revealed by soaring birds and dragonflies

By R. S. SCORER

(Read 17 March 1954. See *Q.J.*, **80**, p. 68)

Instr. Cdr. C. R. Darlington : Dr. Scorer showed us a slide of the albatross, one of the best known soaring birds. But they are maritime birds, so that they will certainly not often meet thermal 'bubbles' of the sort which are found over heated land. Could Dr. Scorer explain by what means the albatross is enabled to soar ?

Mr. L. C. W. Bonacina : Of all the larger British soaring birds the buzzard, which has been mentioned by Dr. Scorer, is the most typical and common, and I should like to ask him whether he has made any special observations of this species in relation to the context. I have often observed buzzards wheeling over the country around Dartmoor in Devonshire, and wondered to what extent these birds could carry out their manoeuvres in the absence of uplifting air currents. Curiously I saw a paper only yesterday by Rudebeck whose observations in southern Sweden seem to answer my query. He says that whereas the common buzzard depends on thermals in 90 per cent of flights, the honey buzzard is much less dependent on the weather, utilizing them in only 70 per cent.

Certainly Dr. Scorer's paper should make those of us interested in bird flight more attentive when we go out into the wilds.

Mr. C. E. Wallington : It would be interesting to know whether or not soaring birds appear to have any particular preference in the direction of their rotation in thermals and approximately what vertical air speeds are necessary to support some of the birds observed.

Mr. R. G. Veryard : On the question of rotation I can say from my own observations, on many occasions, of soaring kite-hawks, that the birds circle in both clockwise and anticlockwise directions – some one way, some the other, at the same time over the same spot.

Mr. P. A. Wills : Mr. Wallington asked if glider pilots made use of circling birds to find upcurrents, and if birds ever returned the compliment. The answer to both questions is affirmative. In South Africa I almost always found that, when I found a thermal and started to circle, I would be joined by first one and then many other vultures; and from an analysis of the times and heights involved I was able to deduce that the vultures make a practice of flying within sight of each other, and thus when any one bird finds a thermal and starts to circle, this is used as a signal for his neighbours to fly inwards and join the lucky one. Similarly if one bird spots a meal, and dives on it, his neighbours take the dive as a signal to fly in and join him, and their neighbours do likewise.

It is I think only the larger birds, who have no natural enemies in the air, who accept a glider in this unperturbed way.

Dr. J. M. Stagg : I have been fascinated by Dr. Scorer's dexterity in fitting Mr. Hankin's observations on soaring flight on to the Scorer-Ludlam theory of thermals. It seems so obvious to see the lightest dragonflies soaring about near the ground and the heaviest adjutant birds soaring at the top where according to the theory the thermals are strongest. But has Dr. Scorer not used some sleight-of-hand ? As the smaller thermals amalgamate into bigger ones with increasing height much of the cooler environmental air must get mixed up along with them, so that if there is no condensation I would expect the thermals to get weaker as they ascend.

Mr. J. Fisher : The necessity for using thermals may limit the northerly distribution of eagles and other soaring birds of prey. It is true that the range of some of these extends north of the Arctic Circle, but when this is so, the northerly elements are normally migratory and withdraw for the winter. There are no thermals in an Arctic winter presumably, and they are somewhat unpredictable in the Arctic regions in summer.

The use of thermals by birds is now widely understood by ornithologists, and Hankin's book is perhaps no longer the best description of the habits and behaviour of soaring birds. Thermal flyways of migratory broad-winged hawks and other big birds of high relative wing area are very well known. To give but a few examples, the sand-dune country at the southern tip of Sweden, Egypt and the Sinai Desert, and Hawk Mountain in Pennsylvania, U.S.A. Large populations of birds use these flyways but they can be outnumbered by the bird-watchers, so interesting is the phenomenon to ornithologists.

Certainly swallows and probably also swifts, and no doubt also dragonflies, haunt thermals simply because they feed on aerial plankton. Once in the thermal they would take the advantage, so that their wing-beats could become slower. But they are not thermal soarers and the relation between their weight and wing area would make it almost impossible for them to be so.

Dr. B. P. Uvarov : We entomologists in our studies of insect flight have to rely very much on meteorologists. In particular, recent work on flight of locust swarms demanded studies of locust behaviour against the background of simultaneous detailed observations on weather. These studies have shown a very close dependence of the changing structure of locust swarms on the dynamics of the air, and, especially, on convection currents. In fact, the observed types of swarms, for which such terms as ' cumuliform ' and ' stratiform ' have been adopted, appear to reflect the structure of convection currents. Should this be the case, it is feasible that entomologists may be able to repay a part of their great debt to meteorologists by providing a supplementary method for direct observations on convection. In any case, here is a fine example how essential is close co-operation between biologists and meteorologists and how fruitful it can be.

Dr. O. G. Sutton : I feel a little disturbed about Dr. Scorer's remark that ' the separate feathers at the tips of the vulture's wings are intended to prevent the formation of the wing-tip vortices.' Any heavier-than-air machine with fixed wings flies by making air rotate (i.e., setting up circulation) and if a bird glides with wings still, it becomes a fixed-wing device and therefore the Lanchester or wing-tip vortices must be formed. Vortices trailing from the wing-tips are essential for lift and I suggest the function of the tip feathers is to provide a very sensitive control of roll, like ailerons.

Secondly, if in ' flexed gliding ' the wing tips are brought well back, we have an approximation to the ' tailless aircraft.' I cannot agree with Dr. Scorer that a bird could fly without a tail, because the tail is essential to maintain fore-and-aft trim, unless the wing tips are brought so far back that the correct leverage to maintain balance is supplied in this way.

Last, may I say something about bubble formation ? In experiments conducted by Dr. Railston at the Royal Military College of Science with small jets of naturally convected air, carefully screened from draughts, it was found that no matter how steady the electrical supply for the ' hot spot,' large disturbances appeared in the jet at irregular intervals. I think these must be the bubbles to which Dr. Scorer refers. I do not know how they are formed, but apparently they cannot be eliminated in a natural convection jet in a calm atmosphere.

Mr. O. W. Neumark : One speaker asked whether there were any means of measuring the vertical velocity of a thermal in which birds were circling. The only satisfactory method is to fly closely behind the bird in a sailplane. Dr. A. Raspet of Mississippi State College (' Performance measurements of a soaring bird,' 1950, *Gliding*, No. 3) used this method for obtaining the aerodynamic characteristics of certain birds. The data were obtained from the instrument readings of the sailplane, from its known performance polar diagram and from photographs showing the relative position and wing aspect of the bird. In this way Dr. Raspet found that the minimum drag coefficient of the black buzzard was 100 per cent lower than that of the cleanest aircraft yet designed.

Dr. Scorer was rather unjust to the albatross when he said that it could not soar in thermals. Sailplanes with spans of 60 ft, with much higher aspect ratios and wing loadings, successfully soar and circle in thermals. The albatross could certainly do likewise *if* it wished to.

I am surprised by Hankin's observations of a ' double dip ' on entering thermals and question Dr. Scorer's explanation. Birds and sailplanes do not usually fly slowly between thermals and on entering most thermals one experiences a sudden increase in airspeed. Yet both birds and pilots wish to circle in thermals at the lowest possible airspeed subject to bank and turbulence. The obvious thing is to pull up, climb and reduce the airspeed and not to dive and gain airspeed on entering a thermal. The reduction of the acceleration, which Dr. Scorer invokes as a motive for the dive or double dip, would, in fact, be achieved by reducing airspeed and not by increasing it.

In Dr. Scorer's written paper it is stated that the gliding angle is increased during ' flex-gliding.' In fact, the gliding angle is reduced (i.e. steepened) by a reduction of span and this

has been confirmed in the case of the black buzzard by Dr. Raspet. The object of ' flex-gliding ' (a horrible term !) is to fly faster than the speed for the best still-air gliding angle.

Dr. R. S. Scorer (*in reply*) : The albatross soars over the ocean by continually passing from air moving at one velocity relative to the earth to air moving at another. If the transition is made in the right direction this can be made always to result in an increase in airspeed of the bird; this is called dynamic soaring. The picture was shown as an illustration of a wing shape adapted to a particular kind of flight. In common with thermal soaring birds a smaller proportion of the wing is occupied by the primaries than in birds that make extensive use of flapping. This does not mean that flapping birds, in particular swallows and similar birds, would not make use of thermals : they do, and can reveal the presence of thermals by soaring, and Mr. Bonacina summarized this point nicely.

I have only once observed a buzzard soaring in a thermal, and have no knowledge of their habits in general. They look like small vultures but on account of their lower wing loading are less likely to find flapping tedious and difficult to maintain.

My casual remark about wing-tip shapes was simply intended to note that there was good aerodynamic reason for having separated primaries at the wing tips in a bird of low aspect ratio. The low aspect ratio is required for flying at low speeds in small circles. On account of their small size seagulls can have a wing more like the albatross because their turning circle in a thermal is a greater multiple of their wingspan than in the case of the vulture.

Many reports speak of birds circling in opposite directions in thermals at one and the same time, but often they go on to say that eventually a majority prevails and, presumably to lessen the risk of collision, they soon all go in the same direction. The sinking speed of a typical soaring bird is certainly no more than for a glider, say about 3 ft per second, because birds can accompany gliders without flapping.

I would have preferred Dr. Stagg to commend the theory that has been fitted to the observations rather than the dexterity with which it was fitted. There is no sleight-of-hand because the smaller thermals only exist close to the ground. If an individual bubble became diluted it might ascend more slowly but in fact several small ones compose the larger one, so that the dilution is reduced.

Thus, if we accept aerodynamic dimensional arguments, the rate of ascent w of a bubble depends on its buoyancy gB, where B is the percentage deficit of density, and a linear dimension R according to

$$w = \text{const } (gBR)^{\frac{1}{2}}$$

the constant being non-dimensional. The buoyancy is proportional to the temperature excess, so that if the amount of heat in the thermal bubble is constant, when it is diluted to size R_1 and buoyancy B_1 we find that $B_1 = BR^3/R_1^3$ and

$$w_1 = \text{const } (gBR)^{\frac{1}{2}} R/R_1$$

and the velocity is decreased.

But the process envisaged creates a bubble of size R by overturning a part of the layer from the ground up to a height proportional to R. Following Sutton (*Q.J.*, **74**, p. 13, 1948) we assume that the buoyancy of the bubble produced is proportional to the average excess, ϵ, of the temperature in that layer over the adiabatic from the top and to its depth, then $B \propto \epsilon R \propto \epsilon z$ where z is the height at which the bubble size is R. Then

$$w \propto (g\epsilon z \,.\, z)^{\frac{1}{2}}$$

But if the amount of heat transported upwards is the same at all levels, and the same fraction of the total area is occupied by thermals, then the product Bw must be independent of height, and so

$$\epsilon^{3/2} z^{2\frac{1}{4}} = \text{const.}$$

Close to the ground other processes are involved but, in the layer which is transporting heat upwards by this process we find that if θ is the potential temperature the above result is obtained if

$$-\partial\theta/\partial z \propto z^{-4/3}$$

and this is not untypical on convection days. If the lapse rate were like this then the smaller bubbles near the ground would have a greater excess temperature but a smaller vertical velocity than the larger ones higher up which they ultimately compose.

Mr. Fisher mentions the eagle, but my impression from Hankin's book was that this was not a bird which depended upon soaring, but used it (though less effectively than cheels) when possible, but was prepared to flap quite a lot if required. If the use of thermals by birds is understood by ornithologists I doubt, nevertheless, whether any has produced a description as revealing of the nature of thermals as Hankin's. It cannot be said that thermals are widely understood by meteorologists.

I would dispute Mr. Fisher's assertion that swallows' wing loading is such as to prevent them from soaring in thermals; indeed I think I have seen them do it. Since thermals contain aerial plankton it is difficult to dissociate them : perhaps it would be more reasonable to say that since the plankton and thermals are found together it would be surprising if a species did not exist which exploited this fact. If swallows learned to use thermals they would be placed at an advantage.

Dr. Uvarov has mentioned the case of locusts. They fly in swarms and the tendency to remain in a swarm introduces a complication in interpreting their movements in terms of thermals; also they are in continuous flapping flight. Nevertheless I agree that a great advance has been made by the close cooperation he mentioned, and further advances are to be expected.

Dr. Sutton requires a bird to have a tail : I merely quoted Hankin's assertion that most of the time ' tail-less ' cheels appeared to be at no disadvantage, but when coming to a perch or on grabbing objects in the air they did sometimes topple over. If the birds' wings were rigid I would agree entirely with Dr. Sutton, but they are not : they are infinitely variable in shape in many different ways and are always under the control of the bird, which must have a very delicate sense of feel. Its servo mechanisms may be able to achieve things far beyond the grasp of a machine designer. The Fauvel Av. 36 is a glider that has no tail and no sweepback and has flown quite successfully, but has reversed curvature on the trailing edge.

Though not asserting that the bubble is the basic element of convection it is profitable until we know more to assume that it is, and evidence of the kind noted by Dr. Railston shows that they are certainly important.

I was not unjust to the albatross : of course, there is nothing in its external structure to prevent it from thermal soaring, but Mr. Neumark has not shown that it is psychologically capable of wishing to do so. Many animals can only learn to do things of which they are capable under the instruction of man.

Hankin did not observe birds performing a double dip on entering thermals; he remarked that a double dip often occurs before a tail jolt. The double dip has been interpreted by me as occurring in the descending air surrounding the bubble cap, and the tail jolt as occurring when the bird crosses the bubble surface into the ascending air. I have discussed this matter with F. G. Irving and it transpires that the result of entering an updraught quickly is to tip up the bird's tail : this is no voluntary movement on the part of the bird, but results from its entry into the bubble at a moment that it cannot exactly predict. Subsequently, of course, if it wishes to ascend, it behaves like an intelligent glider pilot. The glider being larger would not experience the tail jolt so markedly, if at all.

Mr. Neumark is in error over the definition of the gliding angle. It is increased when the bird flex-glides.

I hope that this discussion will bring new interest into bird watching, and new understanding into bird behaviour, but the real objective was, and is, to learn more about thermals, and if we understand birds' behaviour they can be very good indicators to us of air motion.

Report of the Council

for the year 1953

(Submitted to the Annual General Meeting, 28 April 1954)

General

The Council are glad to report another year of steady progress. For the third year in succession there has been an increase over the previous year in the number of Fellows and Foreign Members of the Society and for the first time the total number of Members has exceeded 2,000.

During the summer recess substantial alterations were made to the lecture room, as a result of which the auditorium has been greatly improved and the general appearance considerably enhanced. A complete rewiring of the electrical installation in the whole building was also carried out.

For the first time in the Society's history, a joint meeting of the Society and the American Meteorological Society was held. This was made possible by a generous gift from the Munitalp Foundation in 1952. The meeting took place in Toronto, the Canadian Branch acting as host. Fuller details are given later in the report.

The fourth vacation course held at Malham Tarn Field Centre, in conjunction with the Council for the Promotion of Field Studies, was very successful and 14 students were subsequently elected to Membership of the Society.

Council

From 1 January to 30 September the constitution of the Council was as recorded in the 1952 Annual Report, except that Professor P. A. Sheppard resigned as Editor from 28 January, and was succeeded by Dr. T. W. Wormell.

From 1 October 1953 the following Council took office :

President

Oliver Graham Sutton, C.B.E., D.Sc., F.R.S.

Vice-Presidents

Professor Percy Robert Crowe, Ph.D.
Sir Charles Normand, C.I.E., M.A., D.Sc.
Cameron Darrell Ovey, B.Sc., F.G.S.
Professor Percival Albert Sheppard, B.Sc., F.Inst.P.

Vice-President for Canada

Frank Waldemar Benum, M.A.

Vice-President for Scotland

Alexander Ebenezer McLean Geddes, O.B.E., M.A., D.Sc.

Treasurer

James Strachan Farquharson, M.A., D.Sc.

Secretaries

Howard Latimer Penman, M.Sc., Ph.D., F.Inst.P.
George David Robinson, Ph.D., F.Inst.P. (Librarian)
Richard George Veryard, B.Sc.
Thomas Wilson Wormell, M.A., Ph.D. (Editor)

Councillors

George Arthur Bull, B.Sc.
Henry Charnock, M.Sc., D.I.C., A.Inst.P.
Donovan Chilton, B.Sc., A.Inst.P.
Instr.-Cdr. Charles Roy Darlington, B.Sc., R.N.
Arthur George Forsdyke, A.R.C.S., Ph.D., D.I.C.
Ronald Frith, O.B.E., Ph.D.
Lt. Cdr. George Lawrence Hogben, D.S.C., M.A., D.I.C., Ph.D., R.N.Z.N.
Ralph Edwin Lacy, B.Sc.
Frank Henry Ludlam, D.I.C.
Norman Pye, B.A.
Richard Segar Scorer, M.A., Ph.D.
Mrs. Ann C. Welch, M.B.E.

Meetings

The Annual General Meeting was held at the Society's rooms on 15 April. The Report of the Council and the Annual Accounts for 1952 were adopted. Owing to the sudden death of Lady Normand the Presidential Address was postponed, and in its place Professor Gordon Manley spoke on ' Field evidence of climatic change.' Sir Charles Normand gave his Presidential Address on 17 June, his subject being ' Monsoon seasonal forecasting.'

The Symons Memorial Lecture was delivered on 29 April by Dr. T. W. Wormell who spoke on ' Lightning.' Five Ordinary Meetings were held during the year at the Society's rooms and the Summer Meeting took place on 15 July at the Building Research Station, near Watford.

The Society's third Annual Dinner was held on 20 November. The theme of the after-dinner speeches was ' Meteorology in Education ' and the Guest of Honour was Mr. K. W. M. Pickthorn, D.Litt., M.P., Parliamentary Secretary to the Ministry of Education.

Additional events of interest were : joint discussion meetings with the British Gliding Association on ' Fair weather cumulus ' on 21 January and with the Royal Astronomical Society on ' Stellar scintillation ' on 16 December; the annual popular lecture on ' Weather forecasting ' by Mr. J. S. Sawyer, on 2 December. The latter was so successful that, at the kind invitation of the L.C.C., arrangements were made for the lecture to be repeated early in 1954.

Toronto Meteorological Conference 1953

The Toronto Meteorological Conference 1953, of the Royal Meteorological Society and the American Meteorological Society, was held in Toronto, 9-15 September, and was attended by some 230 Members and visitors. The arrangements for the Conference were made by a joint planning committee of the two Societies, consisting of the following :

Canada. A. Thomson, F. W. Benum, D. P. McIntyre, P. D. McTaggart-Cowan, J. R. H. Noble, R. W. Rae.

United Kingdom. Sir Charles Normand, G. D. Robinson, P. A. Sheppard, R. C. Sutcliffe.

United States. Seville Chapman, K. C. Spengler, C. N. Touart.

The arrangements on behalf of the Society were handled locally by the Canadian Branch and Council wish to record their warm thanks to the Branch for carrying out this formidable task so successfully.

The meeting included symposia on the following subjects (names of respective Chairmen are given in parenthesis) : Ozone and the high atmosphere (Sir Charles Normand), Radiation and other energy transformations (Dr. G. D. Robinson), Arctic meteorology (Mr. Andrew Thomson), General circulation (Prof. P. A. Sheppard), Modern cyclone theory (Prof. S. Petterssen), High-level forecasting (Major-General W. O. Senter), Climatic change (Prof. G. Manley), Micro-climatology and micrometeorology (Dr. H. E. Landsberg), Cloud physics and induced precipitation (Prof. H. G. Houghton).

The Society's delegates included : Sir Charles Normand, Mr. J. K. Bannon, Mr. A. C. Best, Dr. A. W. Brewer, Sir David Brunt, Mr. T. V. Davies, Mr. E. L. Deacon, Mr. A. J. Drummond, Dr. E. T. Eady, Dr. R. M. Goody, Prof. Gordon Manley, Mr. R. J. Murgatroyd, Dr. H. L. Penman, Dr. G. D. Robinson, Mr. J. S. Sawyer, Dr. R. S. Scorer, Prof. P. A. Sheppard, Dr. R. C. Sutcliffe, Dr. T. W. Wormell.

A banquet was arranged at which the speakers were : Mr. Andrew Thomson, the Chairman, Controller of the Canadian Meteorological Service; Sir Charles Normand, President of the Royal Meteorological Society; Prof. H. R. Byers, President of the American Meterological Society; The Hon. L. Chevrier, Minister of Transport for Canada; Mrs. C. J. Beach, Trustee, The Munitalp Foundation and Dr. V. J. Schaefer, Scientific Adviser, The Munitalp Foundation.

Delegates also enjoyed a visit to the Niagara Falls.

It was announced during the conference that the following Members of the Society had been elected to Honorary Membership of the American Meteorological Society: Sir David Brunt, M.A., Sc.D., F.R.S.; Sir Nelson K. Johnson, K.C.B., D.Sc.; Sir Charles Normand, C.I.E., M.A., D.Sc.; Prof. J. Patterson, O.B.E., M.A.; Sir George Simpson, K.C.B., C.B.E., D.Sc., F.R.S.; Prof. H. U. Sverdrup, PH.D.

By arrangement with the American Meteorological Society the Proceedings are being published by the Society in the format of the *Quarterly Journal* and will be available in 1954.

PUBLICATIONS

The inflow of scientific communications for the *Quarterly Journal* and *Weather* was maintained at approximately the same level as in the previous year. 72 papers were submitted for publication in the *Quarterly Journal* during 1953, as against 74 in 1952. 16 of the papers were either withdrawn by the authors or rejected. The average time between receipt of approved copy and publication in the *Journal* was about 3½ months.

The four numbers of the 1953 *Journal* together contained 584 pages, and the series 'Reviews of modern meteorology' was continued.

Council are greatly indebted to the Editor, Professor P. A. Sheppard, and subsequently Dr. T. W. Wormell, the Editing Committee and the proof readers for their services in the production of the *Journal*.

Thanks to the untiring efforts of the Editors, *Weather*, the Society's monthly magazine, continued to prosper and gain new readers. In January the format of *Weather* was improved, and for reasons of economy a change of printers was made in July.

MEMBERSHIP

TABLE A

	Fellows and Foreign Members			Associates	Students	Honorary Members	TOTAL
	Annual	Life	Total				
31 Dec. 1952	1,296	183	1,479	307	90	18	1,894
Since elected	114	1	115	39	24	—	178
Readmitted	1	—	1	—	—	—	1
Transferred	12	—	12	—	− 12	—	—
Deceased	− 6	− 3	− 9	− 1	—	—	− 10
Resigned, etc.	− 47	—	− 47	− 2	− 5	—	− 54
31 Dec. 1953	1,370	181	1,551	343	97	18	2,009

DEATHS

Council record with sorrow the death of the Senior Fellow of the Society, Mr. H. H. Harding, who was elected in 1893. The deaths are also recorded with regret of Dr. L. F. Richardson, F.R.S., who was elected in 1919 and served as a Secretary of the Society from 1921-1924, and of Major H. C. Gunton, M.B.E., F.R.Ent.S., elected in 1936. Major Gunton was a Vice-President and served for many years as Phenological Director.

The following deaths are also regretfully recorded, the dates of election being appended to the names: J. Aspinall (Life Fellow, 1905), Rev. C. Decker (1947), Rev. H. N. Edge (Life Fellow, 1916), J. Fairgrieve (1918), H. A. P. Jensen (1945), H. E. Lowater (1939), M. G. MacLeod (1950), L. Starbuck (1938).

Library

The reorganization of the library was completed during the year. Surplus material, consisting of duplicates or older books of little historical value, was disposed of. The extra space thus made available on the shelves allowed a more efficient display of the books, journals and pamphlets. The preparation of a new author catalogue is in hand. During the year 348 books and pamphlets were issued on loan, and many enquiries were dealt with.

The thanks of the Council are again due to the Librarian, Dr. G. D. Robinson, for the work he has done in the Library reorganization.

Staff

At the close of the year the staff consisted of Mr. M. E. Shoenberg, B.A.; Mr. R. S. Read, I.S.O., M.A., B.Sc., Assistant Librarian; Mrs. A. Wallis, Accounting Assistant; Miss A. Dent, Junior Clerk; together with the part-time services of Miss J. Sampson, Secretarial Assistant; and Mrs. W. Steward, Publications Clerk. Miss V. C. Jones resigned her position as *Weather* Clerk on 31 August, and was replaced by Miss G. C. Jones as from 1 September. The services of the staff and the loyalty they have shown have been of great assistance to the Officers and Council.

Branch and Centres

Canadian Branch

Seven Meetings were held during the year at Toronto, one being the Annual Business Meeting at which the Presidential Address was delivered. The remaining Meetings consisted of lectures or presentation of papers. The Branch continued to be very active and at the end of the year had a membership of 357.

In addition to its normal activities the Branch acted as host and undertook the local organization of the joint American Meteorological-Royal Meteorological Society meeting which was held in Toronto in September. The members of the committee were : President, D. P. McIntyre; Vice-President, D. C. Archibald; Secretary, R. C. Graham; Treasurer, A. H. Mason; Councillors at Large, A. R. McCauley, D. H. Smith, J. M. Leaver, R. A. Hornstein, W. E. Turnbull; Vice-President for Canada, F. W. Benum.

A further important step forward in the work of the Branch was the formation in November of a Montreal Centre, with the following Officers : Chairman, B. A. Power; Secretary, W. S. Harley; Treasurer, W. Hitschfeld. It is proposed to hold five or six meetings a year, at which papers will be presented and discussed. The formation of the Montreal Centre marks an increase in the field of regular scientific meetings, an activity that had previously been mainly confined to the Toronto area. There is a large number of meteorologists in the Montreal area, which is a centre of meteorological interests both in the public-service sphere and in academic circles, and it is expected that the new Centre will be a stimulus to research and to increased membership.

Scottish Centre

Five Meetings were held during the year, four at Edinburgh University and one an excursion to Prestwick Airport.

The Officers were : Chairman and Vice-President for Scotland, Dr. A. E. M. Geddes; Secretary and Treasurer, Mr. J. Paton.

Midland Centre

During the early part of the year the Midland Centre held two meetings, but in view of the small attendance at meetings, it was agreed by the Council in consultation with the Centre that activities should be temporarily suspended. This took effect as from June. The Officers were : Chairman, Mr. A. L. Kelley; Secretary, Mr. P. B. Coley; Treasurer, Mr. H. E. Howle.

Representatives

The Society's representatives on other scientific bodies were :

Royal Society Committees

Gassiot Committee, President; *Scientific Centre Accommodation Committee*, President; *British National Committee for Geodesy and Geophysics*, E. Gold (1949/54), O. G. Sutton (1952/57); *British National Committee for Physics*, F. J. Scrase (1949/54).

Institute of Physics Committees

Board, G. D. Robinson (1949/53); *Journal of Scientific Instruments Advisory Committee*, F. Pasquill; *British Journal of Applied Physics Advisory Committee*, J. S. Forrest.

Cambridge University Meteorological Committee

Sir Charles Normand

Royal Astronomical Society

Geophysical Committee, R. C. Sutcliffe until October, G. D. Robinson from October.

Advisory Committee for the Meteorological Office, Edinburgh

J. Paton (January-February), Sir Charles Normand (March-December).

Awards

Symons Memorial Gold Medal for 1953

To Professor C.-G. Rossby in recognition of his outstanding contributions to meteorology, with particular reference to his work on the fundamental branches of meteorology ranging from small-scale turbulence of the lower atmosphere to the dynamics of the large-scale currents and their relation to the general circulation of the atmosphere.

Darton Prizes for Canadian Members for 1952

1st Prize : Dr. D. P. McIntyre, for his paper ' A technique for wind analysis and forecasting as applied to Victoria, B.C.' (*Q.J.*, **78**, p. 247, 1952).

2nd Prize : Mr. F. W. Benum, for his paper ' Meteorology and aviation ' (*Canadian Branch publications*, **3**, No. 1).

The awards were presented at the Annual General Meeting on 15 April.

Napier Shaw Memorial Prize

Following an appeal to the Membership in 1948 and to Scientific Instrument Manufacturers in 1953, a fund of approximately £900 was raised for the memorial to Sir Napier Shaw. Council agreed that the memorial should take the form of a prize of not less than £100, to be offered at least every three years for the best essay or monograph on a meteorological subject to be chosen by the Society. The regulations for the prize were drawn up and arrangements made for the announcement of the first competition early in 1954, the centenary year of Sir Napier Shaw's birth.

Scientific Activities Fund

During the year the following grants were made :

Dr. E. T. Pierce for (*a*) attending the World Symposium on Sferics at Zurich in March 1953, (*b*) observations on atmospherics at Cambridge and Camborne.

Mr. W. E. Richardson in respect of hire of instruments for continuing observations on Park Fell, near Alston, Cumberland.

Mr. J. A. Taylor for meteorological investigations in south-west Lancashire.

Dr. J. van Isacker for attending the joint discussion meeting on ' Stellar scintillation ' on 16 December 1953.

In addition a grant was approved, for payment in 1954, towards the expenses of the two-day summer meeting to be held in Edinburgh in July 1954.

Interest Groups

A start has been made in implementing the suggestion put forward by Sir Robert Watson-Watt in his 1951 Presidential Address for the setting up of Interest Groups to study the relation of meteorology to human affairs. A group has been formed on ' Education ' under the leadership of Sir Charles Normand, with Mr. J. Paton, Prof. P. A. Sheppard, Dr. R. C. Sutcliffe and Dr. T. W. Wormell.

Revision of By-Laws

At a Special General Meeting held on 18 February the amendments to the By-Laws proposed by Council were approved. The effect of the amendments was to appoint the Librarian as a Secretary of the Society and to reconcile By-Laws 51 and 65, dealing with the procedure for the election of Council, with the Charter.

Finance

The record membership of the Society and the continued popularity of its publications as indicated elsewhere in the Report are reflected in the satisfactory financial position.

During the year approximately £800 was spent on the reconstruction of the lecture room and the rewiring of the electrical installation of the premises.

£525 (as compared with £528 in 1952) has been received as a refund from the Inland Revenue in respect of seven-year deeds of covenant signed by Fellows for payment of their subscriptions. The attention of Fellows is once more drawn to the substantial benefits accruing to the Society under this scheme. It is hoped that more Fellows will be encouraged to participate.

Council record with gratitude an allocation to the Society by the Royal Society Committee of £850 from the Government grant-in-aid of publications fund, with an addition of £350 for the library.

STATEMENT OF RECEIPTS AND PAYMENTS

RECEIPTS

1952 £	£			£	£
			Balance from 1952—		
	750		Westminster Bank—Deposit	1074	
	259		—Current Account	657	
	544		Deposit at Post Office Savings Bank	1067	
1553					2798
	3617		Subscriptions 1953	3880	
	345		Subscriptions former years	697	
	201		Subscriptions 1954 and 1955	254	
4163					4831
45			Donations from Life Members, etc.		22
132			Entrance Fees		140
147			Interest on Investments		162
			Library—		
	250		Grant from Royal Society	350	
	235		Sale of Old Books	—	
	485			350	
			Less expenditure on—		
		188	Equipment and Shelving	28	
		82	Books and Binding, etc.	166	
	270	—		194	
215					156
	2754		Sale of Publications (including £604 in advance)	2888	
	137		Advertisements	291	
	163		*Centenary Proceedings*	23	
3054					3202
1300			Grant from Royal Society (excluding Library)		850
528			Income Tax refunds from covenanted subscriptions		525
1343			*Donation from Munitalp Foundation*		—
	303		Rent of Maisonette	251	
		122	*Less*—Rates	98	
		6	—Repairs	—	
	128	—		98	
175					153
31			Sale of cloud postcards		38
	53		Clarke and Cave copyright fees	51	
	4		*Less* expenses	19	
49					32
			Transferred from other Funds—		
	200		Life Composition Fund	200	
	100		British Rainfall Organization Fund (for administration)	30	
300					230
—			Sale of Lease of Flat, No. 2, in Maisonette		724

We have prepared the above Statement of Receipts and Payments from the books and vouchers of the Society and have obtained all the information and explanations which we have required. In our opinion the Statement is correct and in accordance with the books of the Society, and the minutes of the Council of the Society.

Spencer House, South Place,
London, E.C.2.
17 March 1954.

BALL, BAKER & Co.,
Chartered Accountants.

13035	£13863

FOR THE YEAR ENDING 31 DECEMBER 1953

1952 £	£	PAYMENTS	£	£
		Publications, etc.—		
3878	(5 numbers)	*Quarterly Journal* (4 numbers, Jan.-Oct. 1953) ..	2417	
227		Illustrations	178	
—		*Proceedings Toronto Meteorological Conference 1953* (illustrations)	7	
348		Reprints of Papers	239	
23		Reviews	29	
1030	(two years)	Payments to *Weather* for Members' copies ..	898	
289		Second-hand Journals	163	
5795				3931
		Printing—		
119		General printing	164	
243		Stationery	120	
61	(two years)	*Monthly Weather Report*	76	
423				360
		Office Expenses—		
1438	(including *Weather* 1951 refund of £347)	Salaries and Insurance (excluding Library)	1927	
459		Salaries and Insurance, Library Staff	413	
277		Cleaning	273	
92		Furniture	170	
26		Preparing and Auditing Accounts for 1952 (including *Weather*)	73	
56		Telephone Charges	66	
61		Petty Expenses	28	
768		Packing, Postage and Carriage	627	
87		Modernising Society's Offices	360	
—		Minor Repairs	68	
3264				4005
		Heating, Lighting, Insurance, etc.—		
42		Coal and coke	32	
102		Light and gas	120	
12		Water rate	12	
18		Insurances	18	
174				182
		Meetings—		
120		Expenses and refreshments at Meetings	83	
40		Scottish Centre, Meetings' expenses	40	
15		Midland Centre, Meetings' expenses	3	
175				126
—		Grant to the Council of the Society for the promotion of Field Studies		25
—		Expenditure on Toronto Meeting (balance of Donation from Munitalp Foundation 1952)		1016
		Purchase of Investments—		
—		£2,000 4% Commonwealth of Australia Stock, 1966/68		1990
		Transferred to other Funds—		
2		British Rainfall Organisation for 1952	1	
299		Scientific Activities Fund	—	
43		Life Composition Fund (for 1952)	100	
—		Staff Pension Fund (including £450 for 1952) ..	730	
57		Repairs and Maintenance	75	
5		Napier Shaw Fund	—	
406				906
		Balances in Hand—		
1074		Deposit—Westminster Bank	—	
1067		—Post Office Savings Bank	1119	
657		Current Account, Westminster Bank	203	
2798				1322
13035				£13863

ASSETS AND LIABILITIES AT 31 DECEMBER 1953

1952 £	1952 £	LIABILITIES	£	£
	201	Subscriptions paid in advance	254	
	498	Subscriptions to publications paid in advance	604	
699				858
	106	Rebate payable on Canadian subscriptions	125	
	50	Sundry Expenses	252	
	—	Rewiring Premises..	380	
	450	Due to Staff Pension Fund	—	
	100	Due to Life Composition Fund	—	
	200	Provision for special publications ..	700	
906				1457
11297		Excess of Assets over Liabilities (the excess is exclusive of the value of the Library and stock of publications) ..		11836
12902				£14151

1952 £	1952 £	ASSETS	£	£
		Property, Furniture and Fittings		
		Freehold Property—Estimated value (in 1925) of the Society's house, 49 Cromwell Road		5375
5375				
	830	Furniture and Fittings—General ..	1000	
	209	—Library ..	209	
	1039		1209	
	16	Instruments	16	
1055				1225
		Investments		
	626	Government of Australia 4% Registered Stock 1955-70 £649.1.7 @ 99 xd ..	643	
	197	4% Funding Stock 1960-90 £200 @ 101½	203	
	711	3½% Conversion Loan £900 @ 86 ..	774	
	819	2½% Consolidated Stock £1400 @ 64½ xd	903	
	—	Government of Australia 4% Registered Stock 1966-68 £2000 @ 99½.. ..	1990	
2353				4513
		The above stocks are valued at market prices on 31.12.53 by the Society's brokers		
		Sundry Debtors and Payments in advance		
	150	Subscriptions unpaid–United Kingdom	200	
	700	Canada	800	
	850		1000	
	15	Clarke Collection Copyright fees ..	83	
	404	Publications	633	
	47	*Quarterly Journal* illustrations paid in advance	—	
1316				1716
		Cash Balances		
	1067	Deposit with Post Office Savings Bank	1119	
	1689	Current Account at Westminster Bank Ltd.	187	
	47	In Hand	16	
2803				1322
12902				£14151

17 March 1954

Examined and securities seen,
BALL, BAKER & Co.,
Chartered Accountants.

RECEIPTS AND EXPENDITURE OF SPECIAL FUNDS FOR 1953

	Balance at 31.12.52	Income for 1953	Expenditure for 1953	Balance at 31.12.53
	£	£	£	£
Awards Fund	17	—	—	17
British Rainfall Organization Fund	86	(a) 257	(b) 151	192
Life Composition Fund	265	(c) 288	(d) 528	25
Napier Shaw Memorial Fund	92	325	(e) 374	43
Repairs and Maintenance Fund	—	75	75	—
Royal Society Reprints Scheme	9	26	15	20
Scientific Activities Fund	318	93	99	312
Staff Pension Fund	514	(f) 376	(g) 840	50
Symons Memorial Fund	55	35	51	39
	£1356	£1475	£2133	£698

(a) Includes £100 3% Defence Bonds redeemed; (b) Includes purchase of £100 4% Consolidated Stock 1957 or after; (c) Includes £100 3% Defence Bonds redeemed; (d) Includes £200 transferred to General Account; (e) Includes purchase of £400 4% Consolidated Stock 1957, or after; (f) Includes £280 transferred from General Account, being 15% of 1953 Salaries; (g) Includes purchase of £900 4% Consolidated Stock 1957 or after.

Examined with the books and vouchers of the Society and found correct,
BALL, BAKER & Co.,
Chartered Accountants.

17 March 1954

VALUATION OF INVESTMENTS AT 31 DECEMBER 1953

Market Value 31.12.52			*Market Value 31.12.53*	
£	£	*General Fund**	£	£
		£649.1.7 Government of Commonwealth of Australia		
626		4% Registered Stock 1955-70	643	
197		£200 4% Funding Stock 1960-90	203	
711		£900 3½% Conversion Loan 1961 or after	774	
819		£1,400 2½% Consolidated Stock	903	
		£2000 Government of Commonwealth of Australia		
—		(*a*) 4% Registered Stock 1966-68	1990	
2353				4513
		Scientific Activities Fund		
195		£250 3½% War Stock 1952 or after	212	
79		£100 3½% Conversion Loan 1961 or after	86	
870		£1000 4% Consolidated Stock 1957 or after	935	
1144				1233
		Staff Pension Fund		
624		£800 3½% War Stock 1952 or after	678	
237		£300 3½% Conversion Loan 1961 or after	258	
673		£1150 2½% Consolidated Stock	742	
—		(*a*) £900 4% Consolidated Stock 1957 or after	841	
1534				2519
		Life Composition Fund		
468		£800 2½% Consolidated Stock	516	
632		£800 3½% Conversion Loan 1961 or after	688	
1200		(*b*) £1100 3% Defence Bonds	1100	
—		(*a*) £350 4% Consolidated Stock 1957 or after	327	
2300				2631
		*British Rainfall Organization Fund***		
585		£750 3½% War Stock 1952 or after	636	
2385		£3097.10 3% British Transport Guar. Stock 1978-88	2617	
—		(*a*) £100 4% Consolidated Stock 1957 or after	93	
100		(*b*) £100 3% Defence Bonds	—	
3070				3346
		Napier Shaw Memorial Fund		
276		£350 3½% Conversion Loan 1961 or after	301	
—		(*a*) £400 4% Consolidated Stock 1957 or after	374	
276				675
		Symons Memorial Fund		
514		£650 3½% Conversion Loan 1961 or after	559	
174		£200 4% Consolidated Stock 1957 or after	187	
688				746
11365				£15663

(*a*) Bought in 1953. (*b*) £100 Defence Bonds redeemed in 1953.

* *General Fund* also holds the freehold of 49 Cromwell Road, S.W.7.
** *British Rainfall Organization Fund* also holds the freehold of 62 Camden Square, N.W.1.

Examined and securities seen,
BALL, BAKER & Co.,
Chartered Accountants.

17 March 1954

Proceedings at meetings of the Society

Ordinary Meeting, 17 *March* 1954

The following candidates were declared to have been elected that day by the Council :

Fellows

Eyre, S. Robert, B.Sc., 7 Edgbaston Close, Alwoodley, Leeds.
Garland, Plt. Off. Malcolm Edward, 47 Birklands Drive, Hull, Yorks.
Hadden, Ian Hamilton, Highfields, Kirby Road, Walton-on-Naze, Essex.
Hinds, Mavis Kathleen, B.Sc., 79 Crabtree Lane, Harpenden, Herts.
Moore, Capt. Robert, Jebshun Shipping Co., Bonham Strand West, Hong Kong.
Musson, William Alan John, M.A., Nigerian College of Arts, Science and Technology, Zaria, Northern Nigeria, W. Africa.
Quantick, Herbert Royston, 39 Claremont Road, Bishopston, Bristol 7.
Rawlinson, Michael Joseph, 26 Sanderstead Hill, Sanderstead, Surrey.
Slight, Vivian Crichton, Officer in charge, Airport, P.O. Box 41, Tanga, Tanganyika.
Smith, Dennis Edward, 94 Belsize Road, London, N.W.6.
University of Bristol, Geography Department, Bristol. Nominee : Prof. W. W. Jervis, D.Sc.
Waller, Instr. Lt. Cdr. Montague Alfred, B.Sc., R.N., 47 Elson Road, Gosport, Hants.
Wilkins, James Wallace, M.Sc., 2 Pierce Road, Milford, Auckland, New Zealand.

Foreign Member

Smith, Robert B., 539 East 43rd Street, Brooklyn 3, New York, U.S.A.

Associate

De Mel, Ian David Tennyson, B.Sc., Department of Meteorology, Bullers Road, Colombo 7, Ceylon.

Student Members

Aston, Vivian, 166 Margam Road, Port Talbot, South Wales.
Hepworth-Nicholson, Simon, 106 Gloucester Road, London, S.W.7.

The following papers were read and discussed :

(1) ' The origin of atmospheric nitrous oxide,' by R. M. Goody and C. D. Walshaw, (*Q.J.*, **79**, p. 496).

(2) ' The nature of convection as revealed by soaring birds and dragonflies,' by R. S. Scorer, (*Q.J.*, **80**, p. 68).

Symons Memorial Lecture, 7 *April* 1954

Dr. G. K. Batchelor delivered the Symons Memorial Lecture, his subject being ' Heat convection and buoyancy effects in fluids.'

Annual General Meeting, 28 *April* 1954

Dr. O. G. Sutton, C.B.E., F.R.S., President, in the chair. The minutes of the Annual General Meeting on 15 April 1953 were confirmed and signed.

In presenting the Report of the Council for the year 1953 the President referred to the main items of interest, and in particular to the successful Conference held in Toronto, and to the institution of the Napier Shaw Memorial Prize. He also drew attention to the fact that the total membership had exceeded 2,000 for the first time in the history of the Society.

After remarks by the Hon. Treasurer on the financial section in which he drew attention to the satisfactory financial position of the Society, the President moved that the Report be received and adopted and be printed in the *Quarterly Journal*. The motion was carried unanimously.

Dr. J. M. Stagg then proposed the vote of thanks to the President and Council for the year 1953. This was seconded by Mr. A. J. Whiten.

The President then announced the following awards :

(a) *Buchan Prize*, in respect of the period 1949-1953 :

To Richard Segar Scorer, M.A., Ph.D., for his outstanding contributions to dynamical meteorology as given in the following papers :

' Theory of waves in the lee of mountains ' (*Quarterly Journal*, **75**, pp. 41-56, 1949), ' On the stability of stably stratified shearing layers ' (*Quarterly Journal*, **77**, pp. 76-84, 1951), ' Billow clouds ' (*Quarterly Journal*, **77**, pp. 235-240, 1951), ' Mountain-gap winds; a study of surface wind at Gibraltar ' (*Quarterly Journal*, **78**, pp. 53-61, 1952), ' Sonic and advective disturbances ' (*Quarterly Journal*, **78**, pp. 76-81, 1952), ' Theory of air-flow over mountains—II : flow over a ridge ' (*Quarterly Journal*, **79**, pp. 70-83, 1953) ;
and, in collaboration, in the two papers :
' Bubble theory of penetrative convection,' by R. S. Scorer and F. H. Ludlam (*Quarterly Journal*, **79**, pp. 94-103, 1953), ' Convection in the atmosphere,' by F. H. Ludlam and R. S. Scorer (Reviews of modern meteorology, No. 10, *Quarterly Journal*, **79**, pp. 317-341, 1953).

(b) *Hugh Robert Mill Medal and Prize* for 1954 :

To Charles Kenneth Mackinnon Douglas, O.B.E., A.F.C. for his outstanding contributions to the science of meteorology with particular reference to rainfall, as detailed below :

Since the award to him of the Buchan Prize in 1937, partly for distinguished contributions to our knowledge of the causes of rain, Mr. Douglas has continued to advance that knowledge. A major contribution (with Sir David Brunt, *Mem.R.Met.S.*, 1926, No. 22) included a quantitative discussion of convergence and of the ageostrophic motion of fronts in relation to rainfall. Two later papers (*Quarterly Journal*, 1929, 1934) gave what was probably the first full account of the development of showers and thunderstorms into areas of continuous rain. Other papers in the *Meteorological Magazine* led up to a study of the incidence of thundery conditions and rainfall in summer (with Miss J. K. Moorhead, *Quarterly Journal*, 1946) and of the meteorological conditions in heavy orographic rainfall (with Dr. J. Glasspoole, *Quarterly Journal*, 1947).

In awarding the Hugh Robert Mill Medal and prize to Mr. Douglas the Council of the Royal Meteorological Society are thus honouring the distinction of his researches into rainfall and acknowledging the dedication of a life's work to the advancement of the science of meteorology.

(c) *Darton Prizes* for 1953 :

Home award : Messrs. B. J. Mason and R. Ramanadham for their paper ' A photoelectric raindrop spectrometer,' (*Quarterly Journal*, **79**, p. 490).

Canadian awards :

(1) Dr. W. L. Godson, for his paper ' The evaluation of infra-red radiative fluxes due to atmospheric water vapour,' (*Quarterly Journal*, **78**, p. 367);

(2) Mr. G. W. Robertson, for his paper ' Some agro-meteorological problems in Canada,' (*Canadian Branch Papers*, 1953, No. 2).

Dr. O. G. Sutton, C.B.E., F.R.S., then delivered the Presidential Address, his subject being 'The development of meteorology as an exact science.'

Professor P. A. Sheppard proposed a vote of thanks to the President for his Address and asked that he would allow it to be printed in the *Quarterly Journal*. Mr. F. H. Bushby seconded the vote of thanks. The meeting was declared closed at 6.50 p.m.

Ordinary Meeting, 19 *May* 1954

The following candidates were declared to have been elected on 28 April by the Council :

Fellows

Bass, Michael David, 42 Clevedon Road, Anerley, London, S.E.20 (*transfer from Student Membership*).
Bewley, David K., B.A., 91 Drayton Gardens, London, S.W.10.
Clitherow, John W., M.P.S., 16 Bidston Avenue, Wallasey, Cheshire.
Gibbs, James Edmund, Ph.D., D.I.C., F.R.A.S., 151 Stanley Avenue, Ottawa 2, Ontario, Canada.
Lessing, Rudolf, Ph.D., F.R.I.C., F.Inst.F., F.Inst.Pet., M.I.Chem.E., M.Inst.Gas E., M.Inst. Min.E., 3 Thorney Court, Palace Gate, London, W.8.
Long, Ian Francis, Rothamsted Experimental Station, Harpenden, Herts.
Lowndes, Roy Basil Walter, 52 Brandwood Park Road, Kings Heath, Birmingham 14.
McCulloch, James S.G., B.Sc., Bogside of Brodie, Nairn, Scotland.
McIntyre, Donald Patrick, Ph.D., 315 Bloor Street West, Toronto 5, Ontario, Canada. (*transfer from Associateship*).
Meiklejohn, Jane, Ph.D., Rothamsted Experimental Station, Harpenden, Herts.
Redrup, Capt. Douglas Frederick, D.F.C., Beechwood, Windlesham, Surrey.
Rumsey, John William Raymond, B.Sc. (Eng.), 12 Toulmin Drive, St. Albans, Herts.
Smyth, Instr. Cdr. Eric Thomas William, M.A., R.N., Naval Weather Service, Admiralty, Queen Anne's Mansions, London, S.W.1.
Snihur, Marian Zbigniew, P.O. Box 4, Livingstone, Northern Rhodesia, Africa.

Foreign Members

Frontenac, Theodore, B.S., M.S., 557 West 140th Street, New York 31, N.Y., U.S.A.
Rubin, Morton J., B.A., M.S., 185 Ash Street, Waltham 54, Mass., U.S.A.

Associate

Adeoye, Adebayo Babaniji, Meteorological Station, Katsina, Nigeria.

Student Member

Blackall, Rodney Martin, 78 Crescent Road, Leigh-on-Sea, Essex.

The following candidates were then declared to have been elected that day by the Council :

Fellows

Ansari, Mohammad Mash-Hud, M.Sc., 504, P.I.B. Colony, New Town, Karachi, Pakistan.
Bojdys, Robert, 'Kingston,' 88 High Street, Ramsey, Hunts.
Chadwick, Joseph Geoffrey, B.Sc., 47 Freckleton Road, Toll Bar, St. Helens, Lancs.
Mallard, John Henry, B.Sc., 36 Millway, Duston, Northampton.
Owen, Sqn. Ldr. Douglas, A.R.Ae.S., 72 Avenue de l'Esplanade, Wesembeck, Brabant, Belgium.
Wood, George, 24 Marlborough Road, Westbourne, Bournemouth, Hants.

Student Members

Beadle, Martin, The Round Square, Gordonstoun, Elgin, Moray, Scotland.
Bowater, Frank Thorneycroft, The Nook, Honeyden Road, North Cray, Sidcup, Kent.

The following papers were read and discussed :

(a) ' Observations of rainfall from warm clouds,' by W. A. Mordy and L. E. Eber (*Q.J.*, **80**, p. 48).

(b) 'Turbulence in clouds as a factor in precipitation,' by T. W. R. East and J. S. Marshall, (*Q.J.*, **80**, p. 26). The paper was presented by Prof. P. A. Sheppard.

(c) ' The terminal velocity of snow flakes,' by M. P. Langleben, (*Q.J.*, **80**, p. 174).

Koninklijk Nederlands Meteorologisch Instituut 1854-1954. Centenary celebration volume published by the Royal Netherlands Meteorological Institute, 1954. Pp. 469. N.p.

I suppose most readers of this Journal are aware of the tremendous upsurge in meteorological activity which took place in the middle of last century and which appears to have derived mainly from two very practical matters. The first was the realisation that the application of current climatological knowledge of the winds over the oceans could substantially reduce the times of passage of the great sailing ships of the day – a nice reaction of sail to the upstart steam. The other was the sense that, with telegraphic communication a reality, an attempt should be made to treat current weather synoptically and to provide warnings to shipping of the imminence of havoc-working winds. So it happened that a number of the maritime nations created their state meteorological services about mid-century and the Dutch were in the van of this action. Today they have had the happy thought to celebrate their centenary with a handsome volume describing the history of the service, its present structure and activities, embracing physical oceanography and other branches of geophysics besides meteorology, and concluding with ten original papers on subjects of special current interest by members of today's service.

The Dutch service, K.N.M.I., was founded under Professor Buys Ballot as director, and he with others of like mind gave to meteorology that strongly international cast which has never since deserted the subject. Its last director to have completed his term of office, in 1951, is the equally eminent Professor Vening Meinesz whom this Society was privileged to have as its principal guest at its own centenary celebrations in 1950. The present service has departments of instrumentation, weather service and aviation meteorology, climatology and agricultural meteorology, maritime meteorology, physical oceanography, geophysics (geomagnetism, seismology, and ionospheric physics) and library services. From the illustrations one obtains an impression of a finely equipped service in a becoming set of buildings.

Of the original papers which conclude the volume one may appropriately be picked out for mention here. It is an analysis of the monthly mean temperatures at De Bilt from 1735 to 1940, a series of renown which is owed to Labrijn. Comparison is made with the somewhat shorter Greenwich record from 1764, and there are notable differences as well as similarities in the properties of the respective series. Such detail apart, what a nice idea to discuss the variation in the climate of De Bilt over the last two centuries in the centenary volume of the Dutch meteorological service.

There are appendices giving the names and periods of service of the chief personnel who have made their contribution to the growth of the K.N.M.I. and a complete list of publications by the service. There are also many handsome illustrations, of past directors, buildings, operations rooms, instruments and of states of sky (in colour).

This is a book which tells the reader with some knowledge of meteorology much more than the contents page would suggest. For it is not only a graphic presentation of the evolution of a modern meteorological service and of that service in action; it is so composed that he who runs may read much of the history of his subject, of the events which helped to shape it and what it is striving towards. These things can be appreciated more finely perhaps by inference from a history such as this, of a particular service, than from a direct treatment of matters which sometimes elude one in the grasp.

P.A.S.

Acta Geophysica Polonica. Komitet Geofizyki P.A.N., Warsaw. Volume I, 1953. Pp. 217 in Nos. 1, 2 and 3/4. N.p.

The Acta Geophysica Polonica is one of a number of new scientific journals established in 1953 by the Polish Academy of Science.

The 17 papers published in the first volume are mostly in English, French or German with summaries in Polish. There are two papers in Polish with summaries in English and German respectively and one in Russian with a Polish summary. The papers have a wide scope. The opening paper of Number 1 by T. Kopcewicz of the State Meteorological Institute on new electrical anemographs to give continuous records of wind speed and direction from a cup anemometer and a wind vane creates a very favourable initial impression. Later papers deal with an automatic instrument for measuring rime deposit, atmospheric electricity, determination of run-off, dependence of the psychrometric constant on wind speed, the interesting ideas of Copernicus on the frictional drag between earth and atmosphere, actinometry, probability of heavy rains of specified intensity in Poland and the secular variation of the terrestrial magnetic field.

It is very pleasant to see the appearance in eastern Europe of a new journal publishing papers or summaries in western languages. Our only suggestion to the editors in this respect is that the summaries of their Russian papers should be in a western language.

The introduction states that the Polish Academy of Science has established a Geophysical Institute and a geophysical committee to co-ordinate geophysical investigations in Poland.

We extend our best wishes for the success of the new Geophysical Institute and the *Acta Geophysica Polonica.*

G.A.B.

Memorandum by the National Smoke Abatement Society to the Committee on Air Pollution. National Smoke Abatement Society, May 1954. Pp. 15. 1s.

Owners and users of lethal devices in our society are commonly required under law to be licensed and to have given evidence of their ability to use such devices with proper regard to the public welfare. How far short are we from such standards of personal and public conduct in regard to the lethal device known as the chimney? If the lethal effects of the chimney are apparent only sporadically, the more constant effects of pollution on the dirt in our homes and buildings, on the lack of view over cities, and on the spoiled faces of buildings are there to remind us of the price we pay for modern 'civilization.'

The National Smoke Abatement Society has been working on the public conscience since 1899 to control this monster of our own creation. Recent events moreover have brought home to us very forcibly how great is the need for their work and there is evidence from the provinces and Whitehall that it is beginning to have results in the required direction. The memorandum under notice has in fact been drawn up by the Society to advise the Government Committee of Enquiry on Pollution of steps which it is thought proper to take at this stage to reduce pollution and to obtain a better understanding of the problems involved. For big problems there undoubtedly are, the economics of the situation apart. Our own Society gave some, though not perhaps very penetrating attention to them recently (*Quarterly Journal*, **80**, p. 261, 1954). Meteorologically it is largely a problem of the extreme condition (the reaction of meteorology on public engineering is so often of this kind), and is clearly one in which our public meteorological service can be expected to provide increasingly useful advice if society is ready to make the necessary adjustments in its own operations.

The recommendations of the memorandum are largely concerned with measures to minimize the production of smoke and abnoxious gases at all times and pay no special attention to minimizing the hazards during meteorologically unfavourable conditions. The broader aim is clearly the one to strive for but the latter could well be the more effective during the necessarily long period of conversion of appliances, training of industrial operators and so forth. The recommendations on research could similarly be extended from research 'at the chimney' to the measurement of the amount and nature of pollution near particularly powerful sources – mobile instead of fixed samplers, so that the actual hazards *in* Regent Street and 100 yards or so from the Office of Works furnace chimney in the Borough of X will be known to those concerned.

A meteorologist is not qualified to judge the merits of all the recommendations made in this memorandum, but he can endorse the spirit which inspires it and be ready to play his part in the large effort which remains to be made to build Jerusalem 'among those dark satanic mills.'

P.A.S.

Annual Report of the Director of the Meteorological Office, April 1952-*March* 1953. H.M.S.O. Pp. 56; tables. 2s.

A publication of this nature inevitably bears the stamp of official factual reporting which, at a casual glance from year to year, seems as monotonous as a desert climate. In reality it represents in succinct phrases a huge output of human energy in maintaining a national service of ever-increasing importance.

Apart from research, forecasting and routine work, one of the ways in which the importance of a meteorological service may be gauged is by studying the miscellaneous enquiries, for these, in a sense, afford some idea of what is wanted from it in addition to what it already gives. These may, in fact, be a guide in the consideration of future policy and extension of work. For example, in the climatology section alone, enquiries were received during the year in connection with law cases, water supply, flooding problems, town planning and also with agricultural, industrial and research organizations, both within Government Departments and outside. Among these, assistance was given to British Railways in determining the effect of strong winds in conjunction with extreme temperatures on overhead cables; help was also given to the Rowett Research Institute on the hatchability of hens' eggs; to the City of Leicester in regard to the project of abstracting water from the River Dove. Under the heading of World Climatology, nearly 600 enquiries were received during the year – a large proportion from exporting firms. There was a big increase in the number for temperature and humidity data. Many of the enquiries in the Marine Branch concerned legal investigations in shipping casualties. There was a wide range of agricultural enquiries, some involving major questions of land utilization, others included potato storage, weed control, behaviour and yield of sheep and dairy herds, depth of water pipes, artificial illumination, soil blowing, irrigation, the growth of mushrooms, hops, wheat and so on.

The significance of this incidental work is shown by the nature of the type of enquiries. With an increasing population, a drive to grow more food, the development in scientific techniques and a greater understanding of nature, it is becoming more and more vital that the best information, both climatic and in prediction, should be meted out to those concerned and it is quite obvious, from reading this report, couched as it is in uninspiring official modesty, that the Meteorological Office are fully aware of the great responsibilities imposed on them in collaborating and interdigitating with the innumerable branches of research outside their field and with industry and agriculture on which the ordinary citizen so much depends for his well-being; and the ordinary citizen would do well to read this document and discover for himself the vast field of work covered.

To the man in the street, the daily forecasts would seem to be the only operation that he considers is the function of the Office. It is, of course, the most obvious. As presented now on television, there is

little to be desired in putting over routine forecasts and many intelligent television watchers are becoming 'weather-minded.' But what of the ordinary B.B.C. forecast bulletins? Here it may be there is considerable room for improvement in spite of recent welcome changes. Why should there not be a forecasting station at Broadcasting House so that the forecaster could prepare his forecast on the spot and read it at the allotted time, thus eliminating the present time-lag? This would not only give credit to the forecaster but would also save him the often unjustifiable ignominy of suffering the slings and arrows from critics when a difficult synoptic situation and the time-lag factor have together combined to make his forecast sound absurd at the time of issue – it would, too, be of much greater practical value! A little extra expenditure in public money here might save far larger sums elsewhere through those who rely on the forecasts for practical purposes. In this connextion one might also put in a plea for further information in the restoration of the forecast bulletin before the 9.0 p.m. News. At 5.55 p.m. few ordinary citizens are home from work to listen but it must be that a large portion of the population sit back in the evening to listen to the 9.0 o'clock News. At present they merely hear, incorporated in this, a condensed version of the earlier bulletin; what they want is some intelligent and up-to-date prognostication on which they may base their activities for the following day. This has been somewhat offset by the recent introduction of a summary and forecast at 11.3 p.m. but for many this is late.

With the great demand, with the regrettable continued absence of Airmet but with the high standard of the forecasting – surely something rather better can yet be produced to satisfy the wants of the ordinary man through the ordinary broadcast forecast bulletin? There seems here to be deficiency in efficiency which is incompatible with the rest of the work carried out by the Office or through the Office for the service of man.

C.D.O.

Meteorological Instruments. By W. E. K. Middleton and A. F. Spilhaus. University of Toronto Press. 3rd Edition, 1953. Pp. xi, 286; 192 Figs., 10 tables. £4. 12s.

In the 13 years since the first publication of this standard work on meteorological instruments (see *Quarterly Journal*, **68**, p. 86, 1942) there have been many developments particularly in the field of measurements in the upper air. In this new edition the authors have included descriptions of new instruments and techniques such as radar-wind finding, thunderstorm location, and radar-storm detection and have retained with minor modifications and few additions the previous excellent accounts of the more standardized instruments for measuring surface pressure, temperature, humidity, precipitation and wind. In doing so they have usually confined their description to general principles and their examples to apparatus found in routine use in official Weather Services particularly those of Canada and the U.S.A. They have recognized wisely that it is impossible in a work of this type to give an exhaustive account of all the various research and specialized instruments new examples of which appear in almost every issue of the meteorological journals. A large number of references are given however. This book is therefore an excellent reference book for all practising meteorologists and particularly for students. The researcher in a given field such as meteorological instruments in the aircraft, radar and radio fields would however have to consult the more specialized papers for further information on his subject.

The arrangement of the book has followed that of the previous edition, separate chapters being allocated to the measurement of each of the main meteorological quantities. An extra chapter has been added on special assemblies and systems such as automatic weather stations. Finally an outline description is given of the complete weather instrument envisaged for the future. This would observe at all points in three dimensions and transmit to a collection point where observations would be selected and fed to an electronic forecast computer, the forecast finally being automatically disseminated by radio and other distribution networks. No meteorologist would be engaged in routine procedure from the observation of data to the dissemination of the forecasts but would only be required for specialist interpretation and verification work.

This glimpse of the future will no doubt be criticized by some and deplored by others but it is in line with the general theme of the book that modern meteorological observations and communications are becoming more and more engineering techniques which must be integrated together. That this is so cannot be denied and the last ten years in the British Meteorological Service have seen many changes in this direction – in most radar and radiosonde stations now for instance the technician is a key man and it has been said that the new British station at Crawley will practically work without a meteorologist. Aircraft instrumentation is also moving in the same direction while facsimile transmission and numerical forecasting are starting to complete other links in this chain. It is difficult to see however how the stage between the completion of numerical forecast and detailed forecast of the weather itself can be completed with present knowledge without the intervention of the meteorologist.

The book is excellently produced and has few errors or misprints. The diagrams however are sometimes rather small and difficult to follow. For clarity in describing meteorological instruments there is rarely any reasonable halfway house between photographs of the instrument and diagrams which are completely schematic.

R.J.M.

Die Grundschicht der Troposphäre. By Karl Schneider-Carius. Leipzig (Geest and Portig), 1953. Pp. xi, 168; 69 Figs., 25 tables. 13.50 DM.

With the development during the last decade or so of a comprehensive network of radiosonde stations, synoptic meteorology has tended more and more to treat upper-air observations on a field basis, as exemplified by contour and thickness charts; and the details of the individual ascents plotted on the tephigram or some other form of aerological diagram no longer claim the same share of attention that they once did. Nevertheless, forecasters do well to remind themselves that the detailed vertical structure of the atmosphere is equally as significant for weather as the large-scale horizontal patterns. The appearance of this book, although it deals almost exclusively with well-known individual aspects of upper-air observations is therefore not untimely. It makes no claim to novelty or originality : rather it is to be described as a comprehensive summary of existing knowledge of the vertical distribution of temperature and humidity in the lower troposphere, and its application in synoptic practice.

The numerous diagrams are mostly graphs of temperature and humidity plotted against height, all scales being linear; they are nearly all limited to the lowest five kilometres.

The first two chapters are general in scope and consist of detailed discussions of the various tropospheric layers – the friction and convection layers and inversions, and the associated cloud types. Chap. 3 is statistical, based on upper-air data over long periods at certain selected stations. The following two chapters are concerned with friction, mixing and vertical transport, and their effects on the variation of wind with height. The influence of radiation on the vertical temperature distribution is also dealt with. Synoptic aspects are discussed from the viewpoint of the typical vertical temperature and humidity distributions associated with pressure systems and the various types of fronts. A chapter on climatology deals with the upper-air in relation to the large-scale climatic zones. The book concludes with a short discussion of miscellaneous physical phenomena including atmospheric electricity, refraction and acoustics.

It is a well-produced volume and the diagrams are exceptionally clear. At first sight the layout seems a little confusing; the text runs almost continuously from chapter to chapter with inconspicuous headings, but the comprehensive table of contents, numbered decimally by chapters amply makes up for this apparent defect.

A.G.F.

Methods of Mathematical Physics, Vol. I. By R. Courant and D. Hilbert. New York and London (Interscience Publishers). First English Edition, 1953. Pp. xv, 561; 27 Figs. $9.50.

Courant-Hilbert must be very familiar to many physicists – even if only because other books on mathematical physics often refer to it. Vol. I has now been translated into English by one of the authors; it has been modernized in a few places and there is a supplementary list of reference texts mainly written in English, the original references being mainly to German texts, many of which are not readily available.

When a book on mathematical physics which is nearly thirty years old is translated, one must ask if the ground is already adequately covered by a similar English text and if the methods included in the book are still greatly in vogue. There is no English text known to the reviewer which covers quite the same ground as Courant-Hilbert. Much of the book has not dated, especially the eigenvalue problems of linear differential equations which must always arise naturally from physical problems. On the other hand certain emphases have changed. There is no mention of Heaviside's methods, which are among the most powerful for the resolution of the differential equations of mathematical physics, or of non-linear mechanics which has recently become so important; revision of Vol. II, not yet translated, may include mention of these topics.

The removal of the time-wasting language difficulty makes a fresh delight of reference to Courant-Hilbert and should ensure it a new lease of life; the translation of Vol. II will be awaited with pleasure. The volume itself is strongly bound, well printed and with but few typographical errors. The review copy was marred on several pages by smudged printers' ink but the volume represented a considerable improvement on the last German edition seen by the reviewer. The price is no doubt reasonable in U.S.A. but English students will find the book expensive, especially as most of them will want both volumes; they are worth owning.

E.K.

A textbook of radar. By the Staff of the Radio Physics Laboratory, E. G. Bowen (Ed.), C.S.I.R.O., Australia. Cambridge University Press. 2nd Edition, 1954. Pp. xiii, 617; 350 Figs., 41 plates. 45s.

This book is primarily for the radio student and covers the principles involved in the design of conventional wartime and present-day commercial radar equipments. Apart from the last three chapters, this Second Edition differs from the First only in that the references are now to published and readily available papers, dated up to 1950, and certain errors have been corrected.

Very few pages are likely to interest the meteorologist who is not himself concerned with the manipulation of a radar equipment. A dozen pages are devoted to the effect of the atmosphere upon microwave propagation, covering anomalous propagation consequent upon abnormal temperature and humidity lapse rates near or a little above the surface, and the attenuation and scattering of radio energy by atmospheric gases and meteorological particles.

The last three chapters – Military radar systems, Civil applications of radar, Applications of radar to physical science – have been rewritten for this edition and include a greater proportion of more recent references. In the last chapter two pages are devoted to a summary of the part that radar is playing in providing data for the study of the physics of natural and artificial rain formation; only one reference is made, however, to published work and the authors, who have latterly devoted considerable effort to this study, should be encouraged to compile an up-to-date and comprehensive review of the work in this field which is proceeding in quite a few laboratories.

The book is well printed and bound, and is adequately indexed.

J.E.N.H.

Die Wolken. By R. Süring. Leipzig (Geest and Portig). 3rd edition, 1950. Pp. viii, 153; 12 Figs., 4 plates, tables. 11.80 DM.

This third edition represents an attempt to make up-to-date a book familiar to many readers, which was a good account of cloud physics as it stood in the years before the last war. Unfortunately of the later work which has enlarged and transformed the subject, reference is made only to the contributions of the Germans, which were cut short at the end of the war. The book is a patchwork, with much interesting material not available in other texts, but without a theme.

F.H.L.

Atomic energy; a survey. Edited by J. Rotblat. London (Taylor and Francis), 1954. Pp. viii, 72; 9 Figs., 5 plates, 3 tables. 6s. 6d.

This book contains seven lectures given during January and February 1954 at the University of London, by Sir John Cockcroft on ' Research at Harwell ', Professor Frisch on 'Atomic weapons ', Professor Simon on 'Atomic power,' Dr. Loutit on ' Radiation hazards,' Dr. Pochin ' Medical uses of atomic energy,' and by Professor Lonsdale and Sir George Thomson on 'Atomic energy and moral issues.' It is published by the Atomic Scientists' Association, one of whose objects is to educate public opinion on the technical aspects and potentialities of atomic energy.

F.H.L.

English weathervanes; their stories and legends from medieval to modern times. By A. Needham. Haywards Heath, Sussex (Charles Clarke Ltd.), 1953. Pp. 102; frontispiece, 275 drawings with 37 plates. 10s. 6d.

Weathervanes, or windvanes as the author correctly says he would prefer to call them, are conspicuous and picturesque elements in both town and country landscapes, and we all appreciate their help in the study of local weather. Probably few, however, realize what it is the purpose of this little book to show, that the study of windvanes is of high educational value both on account of their artistic designs, often the work of local craftsmen, and in an old country like ours of their wealth of historical and literary associations.

A chapter explains the scientific construction of vanes including a reference to those on observatories and lighthouses which actuate recording instruments below; but the essence of the volume is the detailed description of the magnificent plates of drawings arranged as between churches, public buildings, business premises and private dwellings.

An idea of the diversity of designs may be gathered from the fact that out of 237 drawings there are only 21 weathercocks and 18 ' ship ' vanes – a splendid example of the latter figuring most aptly on the roof of the Missions to Seamen's Institute in London.

Many English counties are represented in the collection but, as the place of publication would suggest, the larger number are from the beautiful county of Sussex.

It would be difficult, and, indeed, invidious to single out any of these drawings for special mention but in a meteorological journal three may fittingly be selected. A vane about a century old on the tower of St. John the Baptist's Church, Crawley, Sussex, shows a dove with an olive leaf suggesting Noah's dove in a district liable to river floods; one on the Observatory tower, Maidstone, Kent has forms representing sun, moon and stars while its pointer suggests the sun's rays; another on a cottage at Staplefield, Sussex, indicates a desperate encounter with the wind as a young lady's umbrella is being blown inside out with her dog leaning sensibly against it.

These three cases have more immediate reference to the weather than any others, but it is impossible not to refer to the frontispiece of the lion vane of Maxstoke Castle, behind whose embattled towers is a gloriously congested cumulus calculated like Wordsworth's rainbow to make one's heart leap up.

L.C.W.B.

Linke's Meteorologisches Taschenbuch. New Edition, Bd. II. Leipzig (Geest and Portig), 1953. Pp. xvi, 724; 71 Figs., 9 tables. 38 DM.

' Linke ' is being reissued in an edition of 3 volumes. The new editor is F. Baur, and in the second volume now under review ('The Meteorological Computors' Companion ') he has the assistance of H. Israel, H. Landsberg, F. Möller, Therese Olschewsky, P. Raethjen, K. Rawer, L. Sauer, and W. Zimmerschied.

The volume is almost equally divided into text and tables. The former comprises articles on ' Computational and mathematical techniques ' by Baur, Olschewsky and Sauer, ' Equations of dynamical meteorology ' by Raethjen, 'Aerological measurements ' by Zimmerschied, and some ' Climatic meridional cross sections of the atmosphere ' by Landsberg. The mathematical article covers 250 pages. The treatment is at quite an advanced level, and your reviewer, who is no mathematician, would not choose to try to learn a technique from this text. It is doubtful whether a meteorological compendium is really the most appropriate place, for example, for Fräulein Olschewsky's stylish-looking article on the calculus of variations, with its scholarly historical footnotes. Raethjen is more obviously to the point : in 70 pages he gives an exposition of dynamical meteorology which carries the stamp of his personality. ' Practically applicable equations ' are picked out in bold type. The third major contribution to the text, on aerological measurements, covers instruments, methods and accuracy of observation, thermodynamic diagrams, and synoptic upper-air analysis. English speaking readers will probably find it of most interest as a guide to continental practice. Indeed it is likely that such readers will use the book almost entirely for the magnificent collection of tables in the second half. The matter contained in the text is readily available elsewhere, not of course all in one volume, but it is not all going to be required at one time. The tables have only one rival – the recently issued 6th Edition of *Smithsonian Meteorological Tables*, and the only possible answer to the question ' which set shall I use ? ' is ' both.' Each contains material which is not in the other.

The sections on atmospheric electricity (Israel), the ionosphere (Rawer), and long series of observations (Baur), are features peculiar to ' Linke.' The last of these is presumably meant as a help in long-range forecasting. It includes a representation, by six symbols, of the main features of the weather of north and west Germany on each day of an 80-year period, and a table of monthly sunspot numbers from 1749 to 1952. Of the more usual subjects the collection of tables on radiation and atmospheric optics by Möller is outstanding for its completeness and the care with which the latest available sources have been consulted. On the other hand there are dangers in the compilation of each section by its own expert. Unless editorial scrutiny is very strict, inconsistencies may creep in where sections overlap, and your reviewer found two such inconsistencies on the first occasion he used the tables. Stefan's constant is stated on p. 408, under ' atomic constants,' with a probable error of about 0·2 per cent, yet a table of black-body radiation on p. 541 employs a value which differs from this by as much as 2 per cent. The solar constant is stated on p. 400, 'Astronomical and geodetic tables,' to be 1·940 cal cm^{-2} min^{-1}, but on p. 508 Möller advocates Nicolet's preferred value of 1·98 cal cm^{-2} min^{-1}.

These are trivial flaws which should deter no one from buying this book. The price is most reasonable by modern standards (but how hard these standards are to accept), the printing and binding good, and the format convenient. All concerned in its production are no doubt very proud of it, and justly so.

G.D.R.

Grundriss einer Meteorobiologie des Menschen. By B. De Rudder. Berlin (Springer). 3rd Edition, 1952. Pp. vii, 303; 56 Figs. DM. 29.80.

Professor De Rudder's study of the influence of weather and climate on the human organism appears in its third revised edition, incorporating a wealth of new material. Although he modestly calls it an ' outline,' the book is very full and detailed, reviewing and analysing a wide range of research chiefly from Europe and North America. It is primarily a book written for medical men by a medical man, but nevertheless it ought to have a wide appeal to meteorologists, sociologists and geographers. Professor De Rudder appears to be a very careful and conservative worker, avoiding startling snap conclusions, and there are several places where he prefers to leave a question open rather than publish a too insecurely-based conclusion. Unfortunately for the English reader, however, the book is written in a German style which is heavy and full of jargon. The English reader should be well armed with a German-English dictionary and a medical dictionary.

Only those qualified in medical science could offer criticism of the author's findings; but because of the complex style of writing, it may be well to give a brief summary of the main contents. Terms are defined in a short introduction. For all the changes in the atmosphere he gives the name ' meteorism.' The influence and effect of these ' meteorisms ' on human life form the basis of ' meteorobiology,' which can be divided into meteorophysiology and meteoropathology.

The first part of the book is a study of ' weather illnesses ' which occur under certain atmospheric conditions at any season : and there is discussion of the method of statistical measurement of ' groups.' This section also includes a short but excellent survey of modern synoptic meteorology, with a thorough treatment of föhn and inversion phenomena. He studies in detail the influence of ' meteorotropic ' conditions on the

aetiology of a number of illnesses, including rheumatism, ' weather pains ' and wounds, heart and circulatory conditions, and surgical problems of post-operative embolism. In neuroses and mental illness, weather may not be a cause but only a factor in precipitating change.

The effects of weather on healthy humans are next examined. Thus the relationships between blood pressure and barometric pressure, changes in the chemical structure of the blood under differing weather conditions, the influence of weather conditions on the daily rhythm of life, on the nervous system, and on reproductive processes are all discussed. Acclimatisation of varying physical types and their reaction to climatic environments vastly different to their own is studied from experience with the Afrika Korps; but the vital problems of white settlement in the tropical regions are only briefly mentioned. This aspect, which is of the greatest interest to sociologists and geographers and many others, thus loses the opinion of a distinguished medical man.

In the section of the book devoted to the effects of seasonal change the author notes that less work has been done in the last few years in this sphere than on simple weather changes. The methodology is first to establish proof of a seasonal rhythm, then to define the seasonal factors which affect the rhythm, and finally to study the methods of attack of these factors in order to describe the pathogenesis of the illness. Epidemic infectious diseases show wide variations in virulence from year to year and this may be related to yearly seasonal cycles in weather : in Germany it has been noted that 'good potato years are good scarlet fever years,' and the historical study of these variations is reviewed.

The summer-peak illnesses are examined, illustrated by some indifferent maps, and include skin diseases, sun and heat strokes, typhoid infections and infantile paralysis. The book closes with a summary of the possible uses of the science in the hands of the medical profession, and its relevance to the treatment of the great civilisation plagues such as diphtheria, tuberculosis and infantile paralysis that have developed with the growth of modern urban industrial society should recommend the book to a wide public.

N. Pye
R. E. H. Mellor

Maps and diagrams. By F. J. Monkhouse and H. R. Wilkinson. London (Methuen), 1952. Pp. xvi, 330; 199 Figs. 25s.

All who are concerned with the production of maps and diagrams will find this book of real value, for here for the first time is a cartographic treatise in which emphasis is laid on the methodology of compilation and on the techniques of presentation of data. In the general plan of the book, drawing materials and general technical principles are summarized, in an introductory chapter, while the succeeding five chapters are concerned with relief, climatic, economic, population and settlement maps and diagrams. The section that will particularly interest readers of this *Journal* provides a neat summary of the statistical methods that have been employed for the handling and display of climatological data. The value of the section is increased by the inclusion of references to original articles illustrating the application of the various techniques. Students preparing illustrations for dissertations or authors wishing to produce effective drawings for reproduction by line-blocks will find this book of great help.

N.P.

Westward Ho with the 'Albatross.' By Professor Hans Pettersson. London (MacMillan), 1954. Pp. 205; 28 plates and Figs. 21s.

This book is a lively and absorbing description of the Swedish Deep-sea Expedition 1947-48 which was led by the author. While it was mainly written for the general public, the author touches upon various matters of scientific interest, both physical and biological, concerning the deep sea, but, since the book went to press, considerable research has been completed on some of the material brought back so that, in a sense, it is scientifically out of date here and there. Of obvious interest to the meteorologist are the author's comments on the weather encountered at different stages of the voyage, and particularly his account of the work on the deep-sea cores collected by the expedition and the inference of climatic change gleaned from studying the samples from them. It is absorbing and forceful reading, full of colour, especially in respect of the various out-of-the-way Pacific ports of call which the ship made. The author has a rich capacity for self-expression and the book is to be recommended as a popular account of a circum-global marine expedition such as probably has not been undertaken since the days of the famous *Challenger* Expedition towards the end of the last century.

C.D.O.

Obituary Notices

Sir NELSON JOHNSON, K.C.B., A.R.C.S., D.Sc.

Sir Nelson Johnson died on 23 March 1954 at the early age of sixty-two, only some six months after his retirement from official duty.

N. K. Johnson was born in Canterbury on 11 March 1892. His education was first in that city and later at the Royal College of Science South Kensington, where he took Honours in Physics and afterwards engaged in research in astrophysics under Professor A. Fowler, F.R.S. In 1913, Johnson went to the Hill Observatory, Sidmouth as assistant to Sir Norman Lockyer and Dr. W. J. S. Lockyer, until in the First World War he joined the Royal Flying Corps. His experience of the weather problems of the pilot of an aircraft had probably some influence in bringing him into the Meteorological Office in 1919.

In the Office his first posting was to Shoeburyness to meteorological work in connection with gunnery. In 1921, he was seconded to the War Office to take charge of the meteorological section then being established at Porton. This important and relatively independent post called for special powers of organization as well as scientific initiative, and the selection of N. K. Johnson for this position when still a junior officer in the Meteorological Office shows the esteem in which he was already held. At Porton he instituted experimental research into the meteorology of the lower layers of the atmosphere, especially into the vertical gradient of temperature. New recording methods were devised using continuously aspirated electrical resistance thermometers. Johnson had a natural aptitude for this type of experimental work and took great pleasure in planning it. In 1928, the post of Director of Experiments at Porton became vacant and Johnson was promoted to it. This terminated his attachment to the Meteorological Office for the time being. Later he became Chief Superintendent of the Chemical Defence Research Department, War Office. In this capacity, he was responsible for the development of measures for the protection against gas; and it was a tribute to his thoroughness that at the outbreak of war in 1939 the British gas mask provided for the civilian population was acknowledged to be the finest in the world.

In September 1938, on the retirement of Sir George Simpson, Johnson was appointed Director of the Meteorological Office and had immediately to face the planning necessitated by the political crisis which ended in ' Munich.' A year later, the outbreak of the Second World War brought new problems and ever-increasing demands. The scale of these is indicated roughly by the increase in the staff of the Meteorological Office including the R.A.F.V.R. Meteorological Branch, from some 800 at the beginning of the war to over 6,000 at the end. In 1943, Johnson's services were recognized by his appointment as K.C.B.

In 1946, at the first post-war Conference of the International Meteorological Organization, Johnson was elected President and in that capacity presided at the Washington Conference in 1947 when the Convention of the World Meteorological Organization (W.M.O.) was agreed. In 1951 he became President of the W.M.O. for its opening Congress.

When Johnson became Director in 1938 he had been greatly concerned to develop research within the Office. Preliminary plans made then had temporarily to be suspended on the outbreak of war in 1939, though provision was made for the consideration of wartime problems and suggestions; but in 1941 the Meteorological Research Committee, which had been in prospect in 1939, was set up by the Secretary of State for Air; and, so far as possible both within the Office and in other scientific institutions, the provisions for meteorological research and development likely to contribute to military problems were increased. It was not until the major reorganization of the Office in 1947-8, however, that Divisions and Branches within the Office were established to carry out a recognized programme of meteorological research on the broadest lines. Under Johnson's directorship in the fifteen years from 1938, the development of new methods of observation in this country (particularly of the upper air by radio and radar), of new methods of analysis and of research into special problems of physical meteorology can only be described as amazing. No other country, even in this period of special activity, had gone so far or done the job so thoroughly.

In the planning of research, Johnson's attitude was clear and realistic. Whilst valuing enthusiasm directed along proper channels, he was wary of that sort of enthusiasm which urged only in a vague way that ' something must be done ' and might have committed men and money before knowing in what direction they were to start off.

Johnson's personal researches turn largely on the readings made at Porton and at Leafield in the study of the vertical gradients of wind, temperature and humidity in the lower layers of the atmosphere. Later he took special interest in the extended observational programme carried out on a tower at Rye, Sussex. A new result of the observations at Porton, Leafield and Rye is that the mean temperature over 24 hours, on the average for the whole year, *increases* upwards to a height of at least 300 ft. It is in general only at a greater height that the well-known mean lapse of temperature with height is established. Johnson's work is contained in numerous papers of which the best known are :

' A study of the vertical gradient of temperature in the atmosphere near the ground,' *Geophys. Mem.*, London, **5**, No. 46, 1929; ' The vertical gradient of wind velocity in the lowest layers of the atmosphere,' *Prof. Notes Met. Off.*, London, **6**, No. 91, 1948; and, with G. S. P. Heywood, 'An investigation of the lapse rate of temperature in the lowest hundred metres of the atmosphere,' *Geophys. Mem.*, London, **9**, No. 77, 1938.

He also contributed papers to the *Quarterly Journal* of this Society, of which he was elected a Fellow in 1920, and held the office of Vice-President during 1940.

Those who had the privilege of working with and knowing Sir Nelson intimately will understand the aptness of the description ' kind but strong,' with ' reserves of power behind an unassuming manner.' His

endurance, no less than his devotion to duty was outstanding. During the war years he lived in his office in London and was seldom very long away from it except on duty visits. After the war ended, he was faced with further heavy tasks of reorganization within the Office and special responsibilities in the international field. Unfortunately the long period of overwork from 1938 to 1953 seriously undermined his health. He had many interests outside his work and at the time of his retirement the hope of all his friends was that leisure and rest from official duty might restore him to health; but this was not to be.

He is survived by his widow and a son and daughter.

A.H.R.G.

Mr. C. H. PLANT, F.I.M.

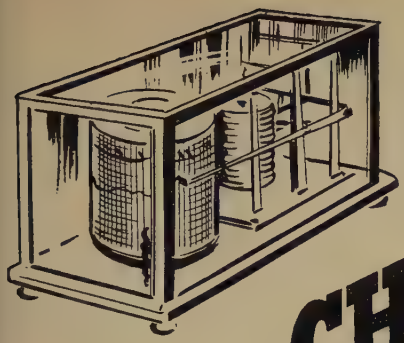
C. Hubert Plant, who held Fellowship in this Society from 1920 to 1924 and again since 1949, died suddenly on 9 February 1953 at the age of 63. A metallurgist by profession, he served throughout his active career on the staff of Thomas Gameson and Son at the Brittannia Foundry, Walsall, Staffordshire, where he had been in charge of the office over a number of years at the time of his death. For about a quarter of a century he was secretary to the Midland Metallurgical Society. Mr. Plant had taken an enthusiastic interest in meteorology since early youth. He long maintained regular records of Walsall weather and contributed monthly summaries of his observations to the local press.

E.L.H.

Mr. A. W. TALBERT, F.R.G.S.

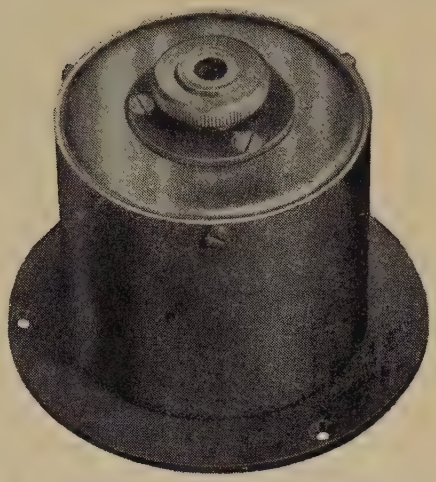
Alfred William Talbert, who died in University College Hospital, London, on 10 January 1954, spent his entire working life of 46 years as draughtsman and cartographer in the service of George Philip and Son, Ltd. He is described by a director of that firm as a master of his craft, well qualified to execute with the highest competence any kind of map; as being always anxious to keep himself fully abreast of the results of modern research; and as possessing a genuine desire to pass on his skill to those whom he trained. From time to time Mr. Talbert's cartographical duties took him by way of climatology into our domain. He became increasingly attracted by the science and was elected a Fellow of the Society in 1942.

E.L.H.



HORSTMANN CHART CLOCK

NEWBRIDGE WORKS BATH
REGISTERED TRADE MARK



Horstmann Chart Clocks are used extensively by the Air Ministry Meteorological Office. Two types are available, one to drive chart drums revolving once in every 24 hours and the other for 7 day rotation. The clockwork movements of both types have a centre spindle drive.

The movements are of robust construction and incorporate very high quality jewelled platform escapements, which ensure extreme accuracy in time keeping.

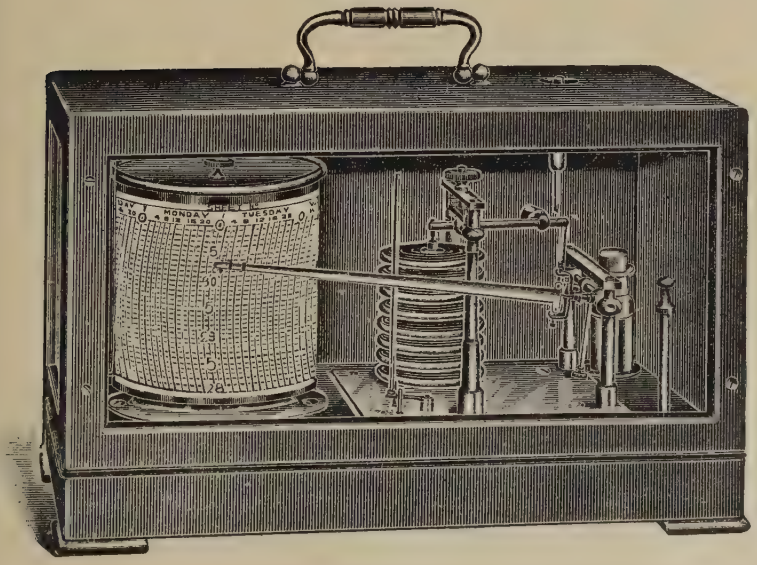
Chart Clocks having speeds other than those mentioned above can also be supplied to special order.

THE HORSTMANN GEAR CO. LTD. · NEWBRIDGE WORKS · BATH Tel.: Bath 7241

T. A. REYNOLDS SON & WARDALE LTD.

40-42 PERCIVAL STREET, LONDON, E.C.1

Telephone : CLErkenwell 6127 Telegrams : Therbarome, London



AUSTRALIAN REPRESENTATIVE :
W. A. AARON
Box 339, G.P.O., Sydney

NEW ZEALAND REPRESENTATIVE :
H. T. JOHNSON & Son, Ltd.
48 High Street,
Auckland, C.1

Manufacturers of

SCIENTIFIC INSTRUMENTS

ANEROID BAROMETERS

THERMOMETERS

BAROGRAPHS, etc.

The Physical Society

PUBLICATIONS

Membership of the Society is open to all who are interested in Physics, and the following publications may be obtained at greatly reduced rates.

1. *The Proceedings of the Physical Society*, published monthly in two Sections : *A*. Atomic and sub-atomic subjects ; *B*. Macroscopic physics.

2. *Reports on Progress in Physics*, published annually, contains critical surveys of various branches of physics written by the specialist for the non-specialist.

3. *The Handbook of Scientific Instruments and Apparatus* compiled in connection with the Physical Society's Annual Exhibition.

4. *Physics Abstracts* (Science Abstracts A) and *Electrical Engineering Abstracts* (Science Abstracts B) are also available at reduced rates to Members.

5. Special publications, critical monographs and reports on special subjects prepared by experts or committees, are issued from time to time.

Further information may be obtained from the Secretary-Editor at the Offices of the Society :

1 Lowther Gardens, Prince Consort Road, London, S.W.7.

BRITISH JOURNAL OF

APPLIED PHYSICS

A monthly publication dealing with the Applications of Physics, especially in Industry.

★

JOURNAL OF

SCIENTIFIC INSTRUMENTS

A monthly publication dealing with the Principles, Construction and Use of Scientific Instruments.

★

Specimen copies of these Journals and particulars of the Institute's other publications will be sent free of charge if this part of the Quarterly Journal is mentioned.

THE INSTITUTE OF PHYSICS

47 BELGRAVE SQUARE, LONDON, S.W.1 *SLOane 9806*

Some Oxford Books

METEOR ASTRONOMY

By A. C. B. LOVELL

Shortly. *Probable price* 50*s. net*

(*International Series of Monographs on Physics*)

This book gives a comprehensive account of the visual, photographic and radio techniques for the study of meteors. This is followed by seven chapters concerned with the number, mass, and velocity distribution of sporadic meteors and by seven chapters on the major meteor showers, including those recently discovered by radio to be active in the day-time sky. Finally the cosmological problem of the existence of the meteor debris in the solar system is considered.

ON THE ORIGIN OF THE SOLAR SYSTEM

By H. ALFVÉN

30*s. net*

(*International Series of Monographs on Physics*)

This volume contains a theory of the origin of the solar system, which was presented in a preliminary form some years ago. Through our increased knowledge in cosmical electrodynamics it has now been possible to bring evidence in support of the fundamentals of the theory and to work it out in greater detail.

THE CLIMATES OF THE CONTINENTS

By W. G. KENDREW

Fourth Edition, 1953 50*s. net*

" The most useful and convenient collection of climatic data available in the English language "—*Nature.*

CLIMATOLOGY

Treated mainly in Relation to Distribution in Time and Place.
Being the third edition of *Climate.*

By W. G. KENDREW

Illustrated 35*s. net*

"An excellent introduction to modern climatology "—*Quarterly Journal of the Royal Meteorological Society*

THE FLIGHT OF THUNDERBOLTS

By F. J. SCHONLAND

Illustrated 15*s. net*

". . . an admirable piece of writing, likely to be regarded as a standard work on all aspects of thunderstorms for many years to come "—*Weather.*

OXFORD UNIVERSITY PRESS

WILSON, WARDEN
AND CO. LTD.

PINE GROVE WORKS,

PINE GROVE,

TOLLINGTON PARK,

LONDON, N.4

Telephone : ARChway 1288. **Cables : Anegraphs, London.**

ANEROID BAROMETERS ★
(DOMESTIC AND MARINE)

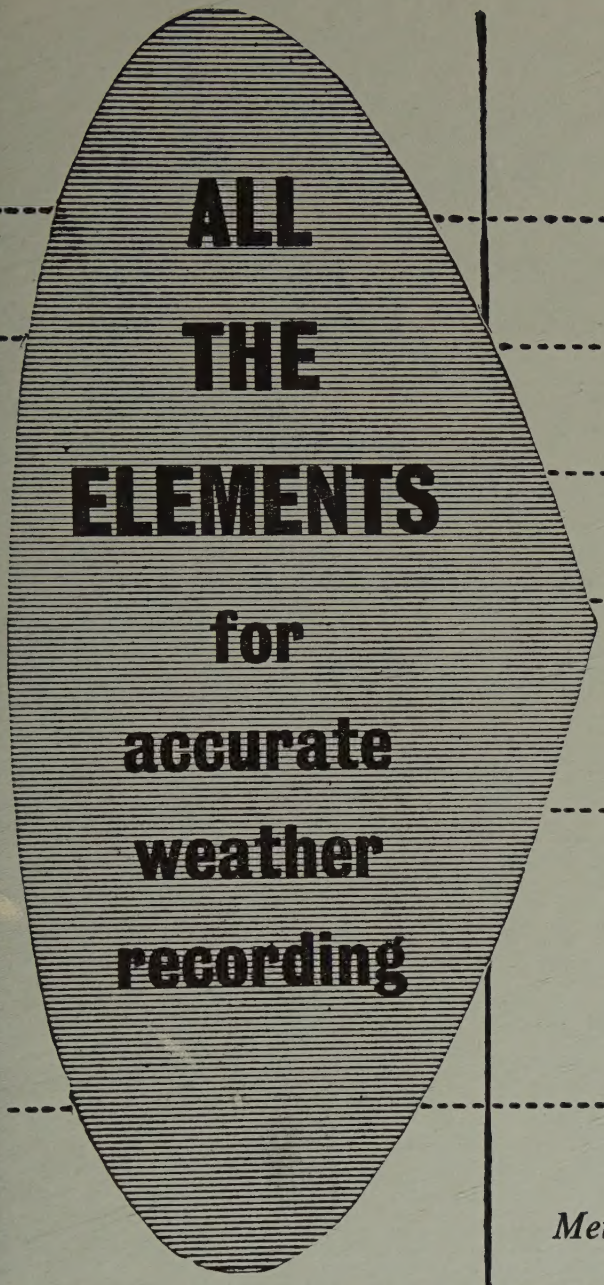
BAROGRAPHS ★

BARO-THERMOGRAPHS ★

★ THERMOGRAPHS

★ HAIR HYGROGRAPHS

★ RAIN GAUGES, etc.



Thermometers

Hygrometers

Barometers

Rain Gauges

Sunshine Recorders

Wind Direction and Velocity Indicators, etc.

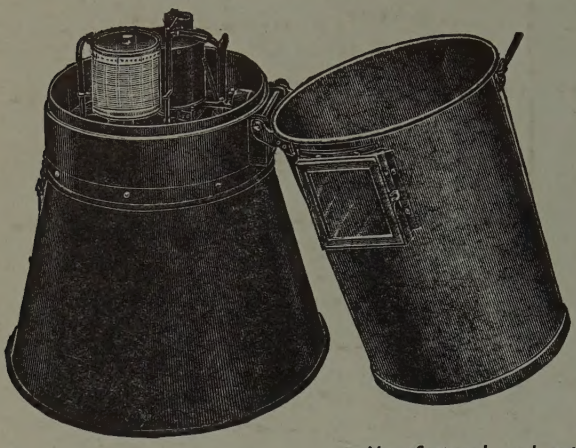
Meteorological Instruments by

NEGRETTI & ZAMBRA LTD

Manufacturers of instruments for the indication, recording, controlling of :— Temperature, pressure, liquid level, volume, specific gravity, humidity, etc.

122, REGENT STREET, LONDON, W.1
'Phone: REGent 3406. 'Grams: Negretti, Piccy, London
Branches : Birmingham, Cardiff, Glasgow, Leeds, Manchester, Nottingham.
Agents or Subsidiaries in most countries.

THE DINES
TILTING SYPHON RAIN GAUGE



This is the very latest type of Self-recording Rain Gauge, fitted with a tilting arrangement which ensures a complete syphoning when the rain water has filled the float chamber.

The Instrument Measures
Overall height – 34″
Diameter of base – 20″

Price and further particulars on application

Manufactured and supplied by

HEATH, HICKS & PERKEN (THERMOMETERS) **LTD.**

NEW ELTHAM, LONDON, S.E.9

Phone : ELTham 3836 *Grams* : "Optimus, Souphone," London

F. DARTON & CO. LTD.

Makers of :

THERMOGRAPHS
BAROGRAPHS
HYGROGRAPHS
PRECISION HAIR HYGROMETERS
ALTIMETER CALIBRATORS
KEW PATTERN BAROMETERS
FORTIN'S PRINCIPLE BAROMETERS
GRASS & SOLAR THERMOMETERS
RAIN GAUGES, etc.

All enquiries to : Established 1834

WATFORD FIELDS, WATFORD, HERTS

Tel. : WATFORD 6019 & 6010

Printed in Great Britain by The Salisbury Press Ltd. Publicity House, Wilton Road, Salisbury, Wilts. (84415)